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THE PROBLEMS OF MENTAL DISABILITY IN ENGLAND

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Nothing like a comprehensive account of the manner in which a great country and people regard, understand, and seek to relieve the mental disorders is to be derived from a seven-weeks' visit during summer holidays. The notes and comments set down here spring from a far less ambitious intent.[1]

A preliminary survey of the rate at which the sciences of life are making their slow entrance into the field of the abnormal in the United States led

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[1] The writer stands heavily in the debt of many persons seen in England and Scotland. So far as he was able to make his way through the tangle of disorder and to assimilate what he heard, read, and saw, he owes the outcome to these generous and obliging men and women. He desires to add to thanks more privately expressed a general acknowledgment. At Maudsley Hospital he consulted Drs. Mapother, Golla, Lewis, Curran, Blackburn, Mayer-Gross, Guttmann and Mann; at the Institute of Medical Psychology, Drs. Rees and Crichton-Miller; at Guy's Hospital, Dr. Gillespie; at Child Guidance Council, Dr. MacCalman and Miss Doris F. Robinson; at the Institute for Industrial Psychology, Drs. C. S. Myers and Miles; at University of Edinburgh and Morningside Hospital, Professors Drever and Collins and Drs. McInnes, Ross, Jones, Cameron and Thompson; at Gartnavel Hospital, Glasgow, Drs. MacNiven and Rodger; among students of disorder and defect in children, Mrs. Melanie Klein, Mrs. Susan Isaacs (City College, University of London), and Dr. Emanuel Miller. Dr. Bernard Hart of Cavendish Square, Dr. William Brown of Oxford and London, and Dr. C. K. Ogden of Cambridge and London were extremely generous in time and hospitality. Among British psychologists consulted were Professors Aveling, Thouless, Wolters, Philpotts, Stevenson, and Wynn Jones. An appointment as American guest of the British Association for the Advancement of Science at Blackpool and that of Vice President *pro tem* of Section J, Psychology, enabled the writer to meet and to hear many British men of science. Arrangements were facilitated by letters from Drs. Henderson (Royal Edinburgh Hospital), Banister (Cambridge), and Bartlett (Oxford). A large collection of documents and reprints, bearing on the history and practice of psychiatry and on medical education, made and classified with great pains by Dr. Edward Mapother, was of the utmost value in acquiring perspective in psychological medicine in England. The informal study here reported was carried out with the aid and support of President Keppel of the Carnegie Corporation.

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the writer to regard the neighboring British field whereupon he was convinced that each of our countries might discharge far more courageously and hopefully its high responsibilities for this grave social problem by making a serious comparison of means and methods and by a frank exchange of views. Wanting the ability, the leisure, and the knowledge to undertake so big a task, the writer planned a brief scouting reconnaissance.

The plan had a much better model set from the other side than it could itself expect to equal. A few years ago Dr. Edward Mapother, Medical Superintendent of the Maudsley Hospital, London, visited some of our mental hospitals, conferred with American psychiatrists, and noted with the expert's eye our provisions for psychiatrical education, training, practice, care, and therapy. Upon his return to England, Dr. Mapother read a paper before the Section of Psychiatry of the Royal Society of Medicine on his "Impressions of psychiatry in America."[2] The present writing is not a return for that informing account. Since its view of the agencies invoked for the disorders is informal, it dwells upon those aspects which are professionally medical only as they occupy a place alongside others, trying to sketch the British plan by which medical organizations and institutions seem to the visitor to fit in with all the other means used toward a single common end.

Many agencies are involved in understanding and relieving mental disorder, quite as many in the British Isles as in America and Continental Europe. Among them stand the medical arts of psychiatry or psychological medicine; hospitals, homes, sanatoria, retreats, and other means of housing and care; institutes—psychiatrical, psychological, and psychoanalytical—for instruction, examination, training, and practice; medical schools; provisions for research in universities and hospitals; university departments of psychology, neurology, biology, biochemistry, and the like, with laboratories for research; organizations for mental hygiene; establishments, committees, councils, and clinics for child guidance; practitioners and consultants; controlling, examining, and licensing bodies; scientific and medical periodicals; reports, textbooks, and critical and systematic writings on research.

Recent increases in the number and range of these agencies have greatly complicated the whole problem of mental deviation toward the abnormal. At the same time, informed opinion on both sides of the Atlantic is inclined to regard the division of knowledge, investigation, labor and responsibility as offering new hope for the mitigation of man's gravest dis-

[2] E. Mapother, *op. cit.*, *Lancet*, 218, 1930, 848-852; see also E. Mapother and A. A. W. Petrie, Impressions of American Psychiatry, *Proc. Roy. Soc. Med.*, 23, 1930, 1197-1209.

ability in life, the misgovernment of his psychological functions and of his behavior.

In alleviation, the historically important agency has been medicine. Since medicine announced its conviction that lunacy, insanity, and madness were *diseases*, the medical arts have assumed responsibility for treatment and relief according to the general principles of diagnosis and therapy. To be sure, a lateral extension of the concept of disease from physical to mental varieties has led to great modifications in it. Except for the demonstration of pathological changes in tissue, the 'diseases' of mind and body have had unlike qualities, histories, descriptions, and contexts. The current tendency —stronger in British than in American medicine—to replace 'disease' by 'disorder' in the mental field is a tardy acknowledgment of a fundamental difference.[3] Certain leaders and parties in American and British psychiatry have held that their primary phenomena were psychological, personal, and social—not pathological (in the bodily sense)—, but they have met great reluctance, among the general institutes and guilds of medicine, to grant the admission, which would imply, of course, a sharp limitation of medicine, not only in therapy but also in understanding and in prevention. Notwithstanding partisan contentions and divisions in England, it appears that the verbal agreement upon 'disorder' marks a real advance toward tolerance and coöperation, a tendency likewise found—to be sure—among certain psychiatrists in the United States, men less keen to follow official designations than to understand and to relieve.[4]

At first sight, the various forms of psychotherapy would seem to represent an alleviative agency competing with medicine. It appears, however, in England as much as in America, that medicine has absorbed as many forms of psychotherapy (the term has a better flavor than 'mental healing') as it has found adjuvant to its physical medicaments, while the others it has condemned as 'irregular' and 'unauthorized.'

One system of mental healing, however—the psychoanalytical—long stood out as an essentially non-medical form of diagnosis and treatment. Doubtless one reason for its long independence is that it was a protestant doctrine and art, a protest against the inadequacy of medical psychiatry.

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[3] "The Committee recommend that . . . the word 'disorder' be substituted for the word 'disease.'" (*Report of the Curriculum Committee of the General Medical Council*, April, 1936, 8.) "In psychological medicine you are not dealing with diseases, but with people, and you can only get an idea of how they are likely to respond to treatment by sizing up the whole situation and assessing their symptoms in the light of their previous history, their present condition, and their personality." (R. D. Gillespie, *Guy's Hospital Gazette*, 50, 1936, 53.)

[4] The present writer does not mean to imply that British psychiatry generally includes in 'disorder' a theory or doctrine of psychogenesis.

Although the modern founder of the doctrine of a disordering unconscious was a doctor of medicine, the medical guild naturally resisted the inclusion in authentic medicine of his doctrine and of his proposed therapy.[5] Of late, however, capitulation has been common. It is the writer's impression that resistance from the side of the great British medical institutions has been more persistent than from official medicine in the United States. Apart from prejudice and tradition, the strongest agent of resistance to any speculative or cultish proposal in this field has been competent research in the sciences of life.

Resistance from the non-professional public is almost negligible in both places. Especially to the unstable, the anxious, the excitable, and the untrained in consistent thinking is the fictional and romantic side of psychoanalysis appealing. Were it not, we should scarcely have seen the recent infectious spread of interest in psychical causes and psychical cures of disorder. Interest, nevertheless, among the ailing and the uninformed has created a demand which has markedly changed the face of psychological medicine. This demand has become all the greater because the specific doctrines and beliefs of psychoanalysis have been amalgamated to cultural trends magnifying man's sentimental and emotional concern with himself and with his personal, private, and family affairs. Whether English practice, education, research, and common sense will succeed better than America has in separating our very great recent gains—largely due to the psychoanalysts—in understanding the biography of the human life-course from bizarre doctrines of a dynamic and libidinous soul remains to be seen.

On the psychoanalytic side of practice must be distinguished the more rigorously freudian Institute of Psychoanalysis, which was organized to supplement the work of the British Psychoanalytical Society, and a much larger 'unofficial' party of medical psychology, which expounds and practices a more or less modified form of psychoanalysis. With the stricter analysts, the medical psychologists share terms and concepts which imply psychogenic origins of disorder, origins which derive from the biographical history of the patient. At times, however, 'medical psychology' in England plainly implies a more inclusive psychotherapy.

The London Clinic[6] of the Institute of Psychoanalysis maintains a Clini-

[5] One view of the essential relation of psychoanalysis to psychology and to medicine has been discussed in the present writer's article, Mind, body, and soul in medical psychology, this JOURNAL, 45, 1933, 577-591.

[6] The London Clinic was incorporated in 1924. Ernest Jones, Chairman, and Edward Glover, Director of Scientific Research (36 Gloucester Place, London, W. 1). The quotations are from its circular of announcement.

cal Research Institute "to further the science of psychoanalysis." It provides facilities "for education and training of medical men and women who wish to practice psychoanalysis." The British Training Committee works in conjunction with an International Psychoanalytical Training Committee. Candidates are required to sign an obligation form "recognizing the Training Committee as the sole competent authority in matters relative to their training, and binding themselves neither to practice psychoanalysis nor at any time to call their practice psychoanalytical practice until sanctioned to do so by the Committee." Training covers at least four years. It includes a personal analysis, lectures on the unconscious, dream interpretation, theory of sexuality, neuroses, technique, child analysis, psychiatry, and mental evolution of the race. The writer gained the impression that the standard medical and psychiatrical education and qualification serve the Institute and its practice more by way of prestige, protection, and authority than in knowledge and training.

Outside the Institute of Psychoanalysis, doctrine and practice in this broad field tend toward some form of eclecticism, presenting certain indigenous aspects and others common also among the more elastic psychoanalysts in the United States. So many are the shades of eclecticism and so elusive the principles and doctrines underlying them that they are difficult to compass and to describe, difficult in one's own community and far more difficult for the observer in foreign lands.

It is obvious, in England as in America, that the sharp issues between the 'standard' psychiatry and freudian analysis should lead at once to partisan enmity and to intermediary forms, practices and beliefs.[7] Indeed a middle ground was long ago prepared in psychopathology and psychotherapy, terms which suggest—in spite of a wide variety of uses—emphasis upon the mental causes in disorder, mental descriptions, and mental means of relief.[8] Although the term 'psychopathology' is sometimes

[7] After distinguishing five main approaches to the psychological problems of mental disorder (analytical, neurological, physiological, constitutional, and ethnological) Nicole complains that British psychiatry has superficially combined, but not really unified, divergent points of view. "They lean on Freud one moment, on Jung the next, then invoke Rivers, just as it suits them, without having done much toward resolving, let alone recognizing, the radical and often irreconcilable differences between the various schools." J. E. Nicole, *Psychopathology: A Survey of Modern Approaches* (2d ed.), 1934, 113-114. If the foundations are simpler in America, that is probably because German psychiatrical traditions have been more faithfully followed (with certain local modifications) and because Freud has been more uncritically believed in.

[8] "There had developed a special field of psychopathology and psychotherapy, in practice and theory strangely separate from psychiatry . . . ; at first largely hypnosis, then persuasion and more and more the freudian and allied methods," Adolf Meyer, *Arch. Neur. & Psychiat.* (Amer.), 30, 1933, 1111.

regarded as an equivalent to 'psychology of the abnormal,' it commonly retains the flavor of medicine and disease and, at the same time, of the psychoanalytical notion of disturbed and disturbing unconscious mechanisms. On the other hand, an account of the disorders which is based upon an experimentally controlled description of the psychological functions of the organism and which takes no more responsibility for therapy and cure than biochemistry takes for relieving indigestion easily escapes the concepts of disease, of pathological conditions, and of all conscious and unconscious agents and energies.[9]

As distinct from this descriptive psychology of the organism, which should stand rather among the fundamental sciences than among the arts of diagnosis, etiology, and cure, England appears just now strong in psychopathology, and especially in various intermediating medical forms of it which heavily tend toward psychoanalytical practice on the therapeutic side. Nicole remarks of England that "there is a growing class of psychotherapeutists whose outlook is largely Freudian without being 'all Freud and nothing but Freud' exemplified by Ross, Rees, Suttie, and others."[10] Doubtless 'war neuroses' and their sequels in English life are partly responsible for this growth.[11]

The attractiveness of Freud for the modern psychotherapist is clearly indicated in the foundation in London of the Institute of Medical Psychology (formerly the Tavistock Clinic), described by its first director as "an organization where psychotherapists of all the schools work alongside of many who claim . . . to be eclectic."[12] Further evidence that the

[9] "The academic psychologists for the most part have enjoyed neither medical training nor extensive opportunities for the study of mental and nervous disorders; while the medical men who have devoted themselves to such studies have had as a rule little training in or contact with academic psychology, a state of affairs well-nigh inevitable and not easily to be remedied in the future." (William McDougall, *Outline of Abnormal Psychology*, 1926, x.) English psychotherapy and psychopathology seem to the present writer to be heavily saturated with the teachings and doctrines of McDougall.

[10] Nicole, *op. cit.*, 116-117. Nicole's pronouncement that "a knowledge of psychology . . . should, as far as possible, become a part of our ordinary life" obviously expresses a conviction common among English psychotherapists. It is common in America, too, as an opinion of the amateur and of the untrained layman. Possibly the recent deluge of popular books and periodicals which dwell 'psychologically' upon private lives, sex, and the more sensational sides of human nature, are largely responsible on both sides of the Atlantic for this view, which is not likely to be shared by those who are responsible for a coherent survey of any large scientific field, whether the field is physics, chemistry, zoology, physiology, or psychology.

[11] See, for example, J. T. MacCurdy, *War Neuroses*, 1918; F. W. Mott, *War Neuroses and Shell Shock*, 1919; William Brown, *Psychology and Psychotherapy*, 1921, and *Mind and personality; an Essay in Psychology and Philosophy*, 1927; Norman Fenton, *Shell Shock and its Aftermath*, 1926; F. C. Bartlett, *Psychology and the Soldier*, 1927; and William McDougall, *op. cit.*, esp. 2.

[12] W. Crichton-Miller, *Psychoanalysis and its Derivatives*, 1933, 6.

chief methods of diagnosis, description, and treatment used at the Institute of Medical Psychology are borrowed from 'psychoanalysis and its derivatives' is to be found in training courses,[13] instructing and training staff,[14] and the character of many publications which represent the institute's methods.[15]

Nothing quite like this Institute, which is primarily devoted to the treatment of 'neurotic illness,' is to be found in America, although many of our hospitals, state and private, minister to the neurotic.[16] Operating on limited means and standing midway between institutional psychiatry and institutional psychoanalysis, it is inevitable that the organization should be critically regarded from both sides. Possibly the practice of recruiting younger men for the standard medical positions in the larger hospitals from Maudsley and a few other well-provided training places in London has put the Tavistock group at a grave disadvantage. It appears that the prestige and resources of mental institutions under the support of the London County Council and of the State have an effect upon professional opinion similar to that of our Federal and stronger State organizations. Whether the more eclectic psychotherapeutical establishments in England also provide less substantial training and less competent practitioners is not a question for foreign and inexpert opinion. With emphasis laid, on the medical side, upon psychotic disorders, as well as upon public care and housing, and, on the other side, upon a long-term and very expensive 'analysis,' there would seem to be a place in any large civilized community, teeming with the neurotic and the *demi-fou,* for research, education, training, and practice in this middle field. There appear to be no intrinsic reasons—apart from the enthusiasms and fashions of our own decades—for making this provision in 'psychotherapeutic' and 'psycho-

[13] A year's training course in preparation for practice in psychotherapy includes lectures (two a week), treatment of patients (nine hours a week), and tutorial supervision.

[14] In 1936 the staff included J. A. Hadfield, Director of Studies; J. R. Rees, Medical Director; H. Crichton-Miller, M. D. Wright, J. Hirsch, C. M. Bevan-Brown, D. Forsyth, G. Adler, A. Maberly, and E. Miller.

[15] Recent publications, which are mainly in the psychoanalytical temper, include *Psychology and Modern Problems,* 1935, and *An Introduction to Psychological Medicine,* 1936. A list of publications by members of the staff is published in the annual report of the Institute.

[16] "Its work aims at the study, alleviation, cure and prevention of those disorders of mind which are not classed as insanity or mental deficiency, but which nevertheless cause profound and widespread suffering and economic disability in the community" (*Report for 1935, Institute of Medical Psychology,* 3). Medical Director Rees has written that "we feel that the psychoneurotic group of the population is so much the larger and that the special needs at the moment are to . . . educate the general practitioner, interest the senior medical students, and gradually help to bring about a psychological point of view in medicine."

pathological' terms which, as we have seen, insist upon uniting the concept of disease with the concept of mental forces.

The present poverty in means for translating clinical actualities into a reasonable descriptive account of the disordered organism has been extremely embarrassing to psychiatry, an art which professes to be based upon the sciences of life; more embarrassing than was the more primitive state of affairs where practice and public had to be satisfied with a half-dozen resounding names labeling the major lunacies. The call to explain the actual exhibits of the clinic has been greatly relieved in our generation —without recourse to the natural and biological sciences—by the injection of speculative systems and doctrines of mental mechanisms alleged to underlie and to induce disordered behavior. Here fall the Austrian and German doctrines of analysis, the French tensions and stresses, fabulous and dynamic instincts and emotions, inherited dispositions, the power of hypnotic suggestion, and the instrument of transference. Some of these psychical constructs are regarded as psychopathic in the causative sense and some of them as psychotherapeutic in the sepse of cure. While no one of these avenues of escape from clinical embarrassment leads through the fields of research, as research is understood in the more stable sciences of man and of nature, they commonly accord with the training of the psychotherapist and are therefore congenial to his intellectual temper and abilities. Once the psychologies of the abnormal are made factually to rest upon the actual life-course of the socialized human organism, with no appeal to theories of the unconscious, of the ego, the racial mind, or the mysteries of transference, all that has been gained by careful observation in the clinic, among children, in experimental and social psychology, and in the biological sciences should be enormously simplified and quite relieved of the magic of medical and philosophical speculations. This relief will be as great for American as for British psychiatry. With greater financial and institutional resources and with the more general use of methods and products of the natural sciences, America's responsibilities are very great.[17]

A more specific comment by the foreign observer may be passed upon education and training. The standard medical education in England has until recently included very little instruction in general psychology, either in the general curriculum or in the professional preparation for medical psychology. Confessions upon this fact made by young men in the medi-

[17] The English reader will not have overlooked a brilliant exposition of the logic and sanctions of psychoanalytic theory contained in Dr. E. Mapother's presidential address (November, 1933) before the Section of Psychiatry of the Royal College of Physicians of London; *Tough or Tender: A Plea for Nominalism in Psychiatry.*

cal profession seem to accord with the statements of recent committees and conferences charged with improving medical education.[18] The Curriculum Committee of the General Medical Council has recommended that "in view of the great importance of a knowledge of psychology in the clinical period . . . an introductory course in normal psychology should be given" under physiology. A Conference of Representatives of the Universities of Oxford, Cambridge and London, and of the Royal College of Physicians (London) and the Royal College of Surgeons (England) likewise has suggested a "short course of lectures introductory to the psychological aspects of medicine." A syllabus of lectures is proposed. It includes about a score of subjects cast in abstract terms which well represent the stock concepts of British textbooks in psychology (instincts, emotions, habits, desires, sentiments, higher mental processes, intelligence, and the like); a heavy and unpalatable dietary to be taken in eight hourly doses! That this course is also to introduce 'medical psychology' appears in the reference to that subject under the 'clinical period' (medical years 3-5), wherein should be added "not less than six demonstrations at a mental hospital."[19] That the student may "be made familiar with the psychological aspects of all patients in the hospital," fortnightly demonstrations may be given by the ward physician or surgeon.[20]

The British Medical Association's Committee on Medical Education also reported on revision,[21] suggesting that throughout the medical course attention should be "directed primarily to health . . . not to disease." "Mental function" should be taught under anatomy and physiology, though perhaps by a specially qualified lecturer, and in the clinical years some account should be taken ("not necessarily as separate courses") of "mental abnormalities." The psychoneuroses are regarded as more important to the student than the psychoses, hitherto chiefly dealt with. For this study the course in mental function suggested for physiology is thought to have prepared the student. A more positive view is taken—following the declara-

[18] Responsibility seems to be divided among the Board of Control, the General Medical Council, licensing bodies, and medical school authorities. Recent demands have included "a higher standard of medical education," more "knowledge of the basic sciences of physics, chemistry, and biology," and the introduction into the first two years of "instruction in psychology, genetics and methods of clinical examination." Provision has been made that "throughout (the course) the preventive and psychological aspects of medicine . . . should receive more prominent attention than has hitherto been common" (*Brit. Med. J.*, (no. 3948), 1936, 452). Resolutions adopted by the General Medical Council on May 29, 1936 will set these and other changes in the medical curriculum into operation on January 1, 1938.

[19] Report of the Conference, 1935, 12. To temper the innovation, the Conference recommends no examination and only eight lectures, which if given at convenient hours should "be listened to with interest."

[20] *Ibid.*, 15.

[21] See *Brit. Med. J.*, Apr. 21, 1934, 192.

tion of the American Commission on Medical Education—of the basal sciences as instruments of education and as training "in the scientific method and discipline."[22]

With these reports before it, a Committee of Management of the two Royal Colleges (both licensing corporations for medicine) brought in recommendations (1935) sustaining the above proposals with respect to a preclinical course in psychology (declaring that occasional questions on this course might appear in the examination on physiology, and approving "occasional demonstrations on the psychological aspects of ill-health").[23] Dr. Mapother thinks that the "principle should from the first be recognized that the pre-clinical teaching of psychology to medical students should be in the hands of a psychiatrist or a psychologist definitely coöperating with a psychiatrist."[24]

After the proposed medical reforms touching instruction in the basal sciences of physics, chemistry, and biology, instruction which is given by teachers who are themselves physicists, chemists, and biologists acquainted at first-hand with their respective fields and with the methods and results of scientific research, medicine may move on, in America as well as in Great Britain, toward a like change in its psychological foundations. Especially in psychological medicine would it seem to be important that whatever psychology is used in practice and taught to students should be both authentic and fundamental. At present there is no guarantee that a psychiatrist medically licensed should know even so much of general psychology as the proposed 'eight lectures' promise to supply. Except for a few medical schools in both countries no provision is now made for a

[22] *Ibid., passim.*

[23] The actual application of all these recommendations to medical and psychiatrical teaching has been competently discussed by E. Mapother (Memorandum concerning the Teaching of Psychological Medicine at King's College Hospital). This writer explains that the improvements have been suggested *au pis aller*, a minimum "to provide for the present situation" where facilities "in most medical schools are far less adequate than at the more advanced universities of Europe and America." The students—as he thinks—"should first be given a thorough grounding in medicine and surgery." He notes "a marked tendency in London now to bring psychiatry as in the best European and American schools into much closer relations with neurology."In this direction—as everyone knows—Maudsley Hospital has led. Dr. Mapother's program in psychological medicine during the entire term of three clinical years obviously looks toward much more thorough and adequate training than has been hitherto provided in England. This program may be compared to (about) two semestral units throughout four years in psychobiology, psychopathology, and psychiatry currently required of medical undergraduates in Cornell University Medical College, New York City. Allowance should be made for the larger American staff (seven professorial members and seventeen instructors) and more ample facilities. In all England there is only one university chair in psychiatry and that was established in 1936.

[24] *Loc. cit.*, 3.

basal psychology unadulterated by philosophy, speculative doctrine, and medicine. If and when an adequate provision of this kind is demanded by medical schools a heavy duty will be placed upon psychologists, a heavier duty than most of them are now prepared to carry. Current psychology must now bear its large share of responsibility for the present state of affairs.

A generation ago a large American university debated the appropriate English to be taught to engineering students, many engineers holding that one of their own number should teach their students English, in order to make the subject both practical and attractive to its students and to deliver future engineers from the unwholesome influence of men professing the English language and literature in the Arts Colleges, professors who were innocent of engineering principles and practices. This historical incident is not unique. Most practical arts and professions, saving those entered through apprenticeship, pass through a stage wherein they attempt to control and to administer in their own fashion all the fundamental knowledge embraced in instruction and training. The common result is a short-cut to many subjects by way of textbooks, second-hand information, and a cram of undigested facts and outworn terms and concepts. In the United States the application of psychology to business, industry, vocational advice, medicine, law, and many other practices is passing through this stage. One common defense of hasty and primitive procedures here is that everyone possesses his own mind, soul, and personality, and may therefore be his own intuitive psychologist. Another is that the freudian dispensation has so brilliantly illuminated 'human nature' and its distresses that the once hidden and mysterious is now open and accessible to every believer or adherent.

In English institutions the doctorate in medicine may be bestowed one or two years after the baccalaureate term in that subject has been completed. In the medical branch called 'psychological medicine' at the University of London the candidate for the medical doctorate must have had an approved hospital attendance, and, besides, examinations, papers, clinical observation, and an essay are also required for the M.D. degree. The candidate is warned that "the standard of the examination for the M.D. degree in psychological medicine is distinctly higher than that of the examination for the Diploma in Psychological Medicine."[25]

The Diploma in Psychological Medicine (or 'in Psychiatry,' Edinburgh) is granted in six British universities (London, Durham, Leeds, Man-

[25] *The University of London, Regulations in the Faculty of Medicine, etc.*, Session 1934-1935, 225, footnote.

chester, Edinburgh, and Dublin) and by the Conjoint (examining) Board of the Royal Colleges of Medicine and Surgery.[26] In the University of London instruction leading toward the Diploma is provided at Maudsley Hospital. Besides the psychoses, the subjects upon which a candidate for the Diploma may be examined are mental retardation, delinquency, and the psychoneuroses. Preparation in these additional subjects is provided by lectures.[27] Lectures in normal and pathological neurology are given in National Hospital, Queen's Square.

The Mental Treatment Act of 1931, providing for the care of 'voluntary' patients in public mental hospitals, has greatly enlarged the advanced study of disorder, as well as the career of the psychiatrist. "In regard to clinical study and teaching the concentration under the authority of the London County Council of all public institutions dealing with every variety, degree and stage of mental disorder, renders accessible such opportunity as probably exist(s) nowhere else in the world."[28] For the disorders the Maudsley (first provided by a gift of Dr. Henry Maudsley and opened in 1923) has become the chief center in London. Several years before the Mental Treatment Act generalized this authority in England, Maudsley admitted voluntary patients. With the clinical material supplied by about 35,000 beds "the hospital might well become the main center of advanced teaching in neuropsychiatry in the British Empire."[29] It provides a five-months' course (January to May) of lectures and practical instruction in preparation for the Diploma. Last year the first part of the course included 24 lectures on the nervous system, 26 lectures on psychological subjects (*i.e.* eight hours on normal psychology, two lectures on physiological psychology, four on applied, four on contemporary schools, two on tests of intelligence, and six on mental mechanisms). The second part, which is psychiatrical, included about 80 lectures and demonstrations, with numerous visits to hospitals and other institutions for the disordered and the defective. Routine and investigative work by psychologists *von Fach* has been done at Maudsley chiefly in the children's department.

The largest and most productive seat of research, clinical and pathological, is also at Maudsley Hospital and at the associated Central Patho-

[26] *The Lancet*, 231 (no. 5896), 1936, 544 ff.; *Brit. Med. J.* (no. 3948), 1936, 493 ff., 499 ff.

[27] *Brit. Med. J.* (no. 3948), 493.

[28] *Medical Superintendent's Report to London County Council on Maudsley Hospital*, 1st Jan., 1927–31st Dec., 1931, p. 3.

[29] *Ibid.*, 1st Jan., 1932–31st Dec., 1935, 5. In the *Annual Report of the London County Council*, 1934 (vol. 6, p. 5) the patients (disordered and defective) under the care of the Mental Hospitals' Committee are given as 37,236 (31st Dec. ,1934). For a copy of this informing report the writer is indebted to Mr. R. H. Curtis, Chief Officer of the Mental Hospitals' Department of the Council.

logical Laboratory of the London County Mental Hospitals. Dr. Aubrey J. Lewis is Director of Clinical Research and Dr. F. L. Golla of Pathological Research. Each heads a large staff of persons of high standing in the sciences of life. Publications are widely scattered throughout the scientific and medical journals and collected from time to time in the *Archives of Neurology and Psychiatry.* The twelfth large volume of the *Archives* (1936) contains forty-four researches in normal and pathological neurology, critical studies of the disorders from the clinic and the laboratory, psychological defects, bodily derangement, physiology (especially metabolism and internal secretion), biochemistry, methods and procedures, pathological states, types and tests of disorder, and course and outcome. The volume represents a cross-section of the most serious lines of research in clinical psychiatry, neuropsychiatry, and systemic physiological derangement, in Europe and America.

The writer's glimpses of men and activities of the Royal Edinburgh Mental Hospital and at Gartnavel Hospital, Glasgow, where large numbers of patients are housed, gave the impression of much more limited facilities in men and equipment, especially in the laboratory type of research which stands nearest the foundational sciences.[30] He lacked opportunity to visit Cardiff, where Dr. J. H. Quastel and his associates are engaged in researches in and around biological chemistry. Publications from that center, however, as well as current psychiatrical opinion throughout England, attest the importance of these investigations. It is only the writer's lack of acquaintance with various researches recently undertaken at metropolitan and provincial hospitals and related institutions that prevents him from according them their just place in British psychiatry and British psychology. When the terrific burdens of housing, care and administration are somewhat relieved by an increase of money and of men trained in the sciences as well as in hospital duty, research in disorder and defect is certain to be greatly extended in Great Britain.

That the amount spent upon research is still very meager is indicated in the remark of Director Golla that under a public expenditure of £7,000,000 a year for maintenance, "the expenditure on research is . . .

[30] At Jordanburn Institute of Psychology in the Royal Hospital, voluntary patients, most of them psychoneurotic, are segregated for observation and study. The psychoanalytic method of interpreting disorder appears to prevail. A psychological clinic for the observation of children is in charge of Professor Mary Collins. Courses in psychology for undergraduate and postgraduate medical students are offered by Professor James Drever and his laboratory staff in the Department of Psychology, Edinburgh University. Strong control of psychological instruction seems to rest in the hands of the psychiatrists. More generally it may be said for both Scotland and England that the chief use made by mental medicine of 'psychologists' is in such technological jobs as testing and examining patients and assisting doctors, routine duties analogous to the examination of body-fluids and pathological specimens. These aides are, of course, not really independent psychologists at all but technicians.

very small when compared with that obtaining in other branches of medicine."[31] Dr. Mapother estimates that a 'team' of researchers, composed of a psychologist, an anatomist, a biochemist, an expert on infective processes, and a clinician might cost £8,000 a year. He holds that "properly critical research will never come from those who are in practice or bound for it. The only hope for the sort of dispassionate long-term research which psychiatry needs is the creation of teams of investigators guaranteed careers, with rewards comparable to those which similar abilities would bring if otherwise marketed. Most of the members of such teams should not primarily be psychiatrists at all, but real experts in various branches of science, who had brought its technique to the service of psychiatry, and then received enough training in this to enable them to see its problems."[32]

While medical psychiatry in America probably regards itself, on the whole, as more self-sufficient for the independent solution of scientific problems, many look with confidence to a fuller use in the disorders of the instruments and the products of research in the sciences of life.[33]

In England the disorders and defects of childhood touch our subject on several sides. In passing, the writer noted provisions for the observation and testing of mental deficiency and for the description of disordered behavior in the young, together with its sequels in older life. Besides special arrangements for the examination, study and treatment of children (*e.g.* at Institute of Medical Psychology, Guy's[34] and the Maudsley in London, and at the psychological clinic at Morningside in Edinburgh), the Child Guidance Council was especially organized for the skilled treatment of problem-children and of early symptoms of nervous disorder. The plan of the Council has been to advertise "the need for Child Guidance Clinics, towards training personnel for such clinics, towards encouraging and conducting research in this field, and towards promoting the better understanding and management of children in general."[35]

The London Child Guidance Clinic "has become the recognized center for training psychiatrists, psychologists and social workers in clinical work with children,"[36] while the Council has become a general clearing house

[31] *Annual Report of the London County Council,* 1933, vol. 6, p. 63. Elsewhere Dr. Mapother has declared that "we need at leading universities about half a dozen Institutes of Psychiatry with access to early cases (*Tough or Tender, etc.,* 1711).

[32] E. Mapother, *Mental Hygiene* (Engl.), Oct., 1935.

[33] *Cf. The Problem of Mental Disorder,* N.Y., 1935.

[34] For an account of the provision for children at Guy's Hospital see *Report of the Institution and Development of the Child Guidance Clinic, 1930-36,* by R. D. Gillespie.

[35] *Annual Report of Child Guidance Council for 1935,* 4.

[36] *Loc. cit.*

for many children's clinics and for the agencies of mental hygiene at large in the community. It has also advanced graduate training for social workers. Dr. MacCalman of Glasgow has succeeded Dr. Moodie as General Secretary. The English and Scottish Councils are affiliated. Although the London Council has received financial support from British and American sources, it has obviously suffered for want of such abundant funds as have been placed at the disposal of the National Committee for Mental Hygiene in New York and other American agencies of sanity. In noting the effective means for the study of disorder and defect among children, the psychoanalysts should not be omitted. It is obvious that both the stricter and the more eclectic analysts have performed a very great service for the understanding of developmental difficulties from infancy onward, through earlier and later childhood and adolescence.[37]

The summer holidays, the absence of men from their posts, and the limitations, personal and temporal, of the visit stood against a satisfactory survey of the British universities with respect to their part in the understanding of, and instruction in, mental disorder. Most, if not all, of the larger universities in Great Britain include medical faculties, conduct schools of medicine, and grant medical degrees. All are subject to the General Council of Medical Education and Registration of the United Kingdom, to conjoint examining boards, and to general licensing, registering, and qualifying bodies.[38] This coördination of the university medical schools is unlike American practices, wherein the individual university operates in fair independence under its charter and under the general laws of the state in which it is resident. In the United States, standardization is sought by way of classification and grading partly under the control of state licensing boards and partly under conjoint action by the Association of American Medical Colleges and the American Medical Association.[39]

[37] Among the major publications in this field stand *Intellectual Growth in Young Children*, 1930 and other books by Susan Isaacs of the University of London.

[38] For a condensed review of medical powers and practices in Great Britain see *The Lancet*, 231, (no. 5896), 1936, 475-525. A good idea of a year's work in routine and research from the 101 public mental hospitals under the Board of Control (an executive and governing body provided by Parliament in the Mental Treatment Act of 1930) will be found in this Board's *Annual Report for 1934*, (pt. i, 73-87; pt. ii, 1-164). The number of disordered persons under state care in England and Wales exceeds 150,000, nearly a fourth of them 'voluntary' patients.

[39] Americans will find a useful digest of British medicine in a report of the 'Commission on Medical Education' upon *Medical Education and Related Problems in Europe*, 1930, 9-53. The final and general report of this Commission, which was organized (1925) by the Association of American Medical Colleges, was published in 1932. The Commission advises, as the reformative prescriptions of the British do, premedical preparation in the fundamentals of physics, chemistry, and general zoology "for the specific purposes of medicine" (p. 276). With respect to the needs

To the foreigner, the Universities of London, Oxford,[40] and Cambridge appear to be by far the most important in the whole group for education and research in psychiatry. The report (1935) of a Conference of representatives of these universities and of the two Royal Colleges (Physicians of London and Surgeons of England) has been mentioned above. In recommending changes in the medical course, this Conference declared that "the ideal approach to clinical study is by a University training leading to a degree in arts or science." The advantages here are said to be the extra time devoted to preparation (6–7 years at Oxford and Cambridge), "contact with men working in other subjects" and with "teachers whose concern and interest in life are the development of their own branch of knowledge," and the more thorough study of the natural sciences, whence men bring "new ideas and a fresh outlook."[41] There are indications that those medical subjects which touch upon mental disorder and deficiency stand to profit along with other medical branches; though the present usual five-year course in medicine probably leaves the licentiate in 'Psychological Medicine' about as innocent of the spirit and the method of research in the sciences of man and of nature as most American schools of medicine leave its young psychiatrist.

If the temper and the extent of serious publications upon the disorders and deficiencies are to be taken as an indicator of the quality of the best men and the most productive research, then Americans will find much to admire and to emulate in the British literary output. Admiration might

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of psychiatry, the recommendations are more vague and much less forward-looking than the British. The report declares that the present understanding of the factors producing "many of the mental, emotional, and functional disorders [note the generic classes!] is comparable to that which existed in regard to general medicine fifty years before the introduction of accurate, measurable criteria of study which transformed medicine and surgery from mysticism. . . ." (214-215). Urging the need of "sound basic knowledge" of neuroanatomy, embryology, neurophysiology, and endocrinology, there is no hint of a demand for a psychological description of the disorders or for a biographical history of the disordered organism. One probable reason for this important omission is that present medical teaching in these subjects (especially in neurophysiology) is saturated with a pseudo-psychology which the critical and informed psychologist utterly rejects as belonging to the times of "mysticism, empiricism, and guesswork." (Cf. The psychologist's uses of neurology, this JOURNAL, 49, 1937, 233-264.) The Commission's insistence upon the "urgent need of at least a few fundamental studies" in psychiatry, when as a matter of fact, more than 'a few' were at the time of the report completed or in progress, taken together with its complacent hope in "sound psychiatric training" raises a question upon the Commissioners' apprehension of the actual state and requirements of the subject of disorder, a subject which lies—in the view of the Commission—entirely within the limits of 'the medical sciences' and 'medical practice.' Current information on American medicine is to be derived from the *Proceedings of the Annual Congress on Medical Education, Licensure and Hospitals* (Amer. Med. Assn., Chicago).

[40] Plans for the recently established Institute of Experimental Psychology at Oxford, which is to include research in the field of medical psychology, have been described by its first director, Dr. William Brown, (*Brit. Med. J.*, May 30, 1936, 1121).

[41] *Report*, 7.

well increase upon the discovery that British research, writing and publication have to contend against exhausting demands of practice, hospital duty and administration, and against extremely meager financial resources. Nothing, alas! like the relative large sums available for research and publication in our country is to be found in England.[42]

Encouragement is to be found in the new drift in biological research now observable in a few places in Great Britain where the problems of disorder are under serious investigation. The incorporation of the methods and results of this research in physiology, neurology, descriptive psychology, biochemistry, and other foundational sciences into the materials of instruction and into the fiber of the younger men during the years of medical training is bound to have its effect. While this infiltration into medicine from the sciences is still fairly slow in America, resources and men have been found for establishing and continuing research in such places as City Hospital, Harvard Medical School, Massachusetts General and Myerson's 'team' (in Mapother's sense) in Boston, at the Institute of Human Relations (Yale), in Rockefeller Institute, New York State Psychiatric Hospital, and other foundations in New York City, among competent groups in Philadelphia, in Jacobson's Physiological Research Laboratory and the Institute for Juvenile Research, Chicago, and in Washington University at St. Louis. British psychiatry has of late been generous in acknowledging a debt to American clinical teaching; several of her abler young men have gone to school at Phipps Clinic and have inspected our hospitals and training institutes. Now we may be entering upon a new term of years wherein a free interchange of researches and investigations from both directions will rapidly advance our comprehension of the disorders.

Free interchange should not, however, be narrowly limited to 'biological subjects' taken in the narrower sense. Whether sane or disturbed, man lives with and by his fellows and under human institutions. Absolutely essential to our common problem, therefore, is a clearer and fuller factual description of the socialized life-course and of the psychological, social, and biographical factors which disrupt sanity and introduce disorder. Much more research here is necessary if students of the disorders are to exchange fanciful and speculative belief and doctrine for scientific principle and sound explanation.

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[42] The Rockefeller Foundation and General Education Board have allotted $148,000,000 "to improve the quality of medical education." Although this huge sum was spread over the entire earth, a fairly small share has found its way to the British Isles for the scientific study of mental disorder. *Cf.* President Fosdick's *A Review for 1936*. p. 22.

THE NEUROLOGICAL LEVEL OF THE FACTORS UNDERLYING TIME-ERRORS

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The aim of the present study is to obtain evidence concerning the neurological level of the processes on which time-errors depend. The study deals more particularly with the negative time errors and with the question whether these depend upon cortical functions.

Recent years have witnessed a considerable revival of interest in the conditions and the theories of those constant errors which are referable to the temporal order of presentation.[1] The phenomena themselves have been known since the early days of psychophysics. When two stimuli of equal intensity are successively presented with a time-interval greater than ca. 2 sec., there is a tendency under certain conditions for the second object to be reported as more intense than the first. From a large number of such comparisons, therefore, the number of reports 'second more intense' will exceed the reports 'second less intense.' This effect of temporal order is known after Fechner as a *negative time-error*. As the time-interval increases, the size of the negative time-error has been said also to increase.[2] In relatively short time-intervals (less than ca. 2 sec.), a positive time-error occurs; *i.e.* there is a tendency for the second stimulus-object to be reported as less intense than the first.[3] Under special conditions of presentation these results may be modified.[4]

In explanation of these effects, Fechner[5] and others accepted the doctrine that

* Accepted for publication February 7, 1937. These experiments and those upon space-error (to be reported later) were performed in 1930-31 in the Berlin Psychological Laboratory during the writer's residence there as National Research Fellow. Appreciation is expressed to Professor Wolfgang Köhler for suggestions made in connection with these studies.

[1] Time-errors have been observed in connection with various properties of perceptual experience, such as intensity, quality, duration, and extent. The present discussion is limited to those associated with the aspect of intensity.

[2] J. Borak, Über die Empfindlichkeit für Gewichtsunterschiede bei abnehmender Reizstärke, *Psychol. Forsch.*, 1, 1921, 374-389; W. Köhler, Zur Theorie des Sukzessivvergleichs und der Zeitfehler, *ibid.*, 4, 1923, 149-153. K. Fodor und L. Happisch, Über die Verschiedenheit der Unterschiedsschwellen fur den Geschmaksinn bei Reizzunahme, *Arch. f. d. ges. Physiol.*, 197, 1922, 337-347; Die Bedeutung der Zeit zwischen zwei Vergleichsreizen bei Bestimmung von Unterschiedsschwellen. Untersuchungen am Gesichtsorgan, *ibid.*, 201, 1923, 369-375.

[3] W. Köhler, Zur Theorie des Sukzessivvergleichs und der Zeitfehler, *Psychol. Forsch.*, 4, 1923, 115-175.

[4] O. Lauenstein, Ansatz zu einer physiologischen Theorie des Vergleichs und der Zeitfehler. *Psych. Forschung*, 17, 1932, 150-170.

[5] G. T. Fechner, *Elemente der Psychophysik*, 1, 1860, 88-92; P. Stärke, Die

the comparison of two successive stimulus-objects was based on the comparison of the second with a memory-image of the first. The occurrence of a negative time-error and its increase with time-interval were explained by assuming that the intensity of the memory-image gradually fades. This theory persisted for many years, but was gradually dropped as a result of phenomenological observations which showed that successive comparisons usually occur in the complete absence of a memory-image of the first object.[6] As recently as 1923, however, we find this doctrine again proposed, by Fodor and Happisch, to explain their observations of time-errors occurring in the successive comparison of tastes and of visual intensities.[7]

As a substitute for the memory-image doctrine, Köhler revived the notion of a physiological trace,[8] applied here as a persisting after-effect in the brain of the first stimulus. Comparison is then assumed to depend upon a 'transition' from the 'trace' to the effect produced in the brain by the second stimulus, which is assumed to be localized in the same region as the trace. The change in magnitude and direction of time-error with time-interval is then accounted for in terms of hypothetical changes in the brain-trace. That property of the trace which represents intensity is assumed to run through a continuous course. Up to intervals of ca. 2 sec. it exceeds its initial value and beyond this critical point it gradually falls away. The course of a time-error curve is thus explained by assuming a parallel course in the property of the trace representing the intensity of the original experience. The physical-chemical events in the brain suggested by Köhler as the correlate of these changes need not now be considered.

Since the formulation of this hypothesis, additional facts on the conditions which influence the time-errors have been obtained. Lauenstein[9] investigated the influence on these errors of the temporal background against which the stimulating object appeared, Pratt[10] the influence of interpolated stimuli, Woodrow[11] the influence of absolute level of standard stimuli, and Needham[12] the influence of repeated experi-

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Messung von Schallstärken, *Phil. Stud.*, 3, 1886, 262; A. Lehmann, Kritische und experimentelle Studien über das Wiedererkennen, *ibid.*, 7, 1891, 169.

[6] G. E. Müller and F. Schumann, Über die psychologischen Grundlagen der Vergleichung gehobener Gewichte, *Arch. f. d. ges. Physiol.*, 45, 1889, 37-112; G. E. Müller, *Abriss der Psychologie*, Göttingen, 1924; F. Schumann, Beiträge zur Analyse der Gesichtswahrnehmungen. III. Der Sukzessivvergleich, *Zsch. f. Psychol.*, 1902, 30, 249-291, 321-339; M. Bentley, The memory image and its qualitative fidelity, this JOURNAL, 11, 1899, 1-48; G. M. Whipple, An analytic study of the memory image and the process of judgment in the discrimination of clangs and tones, *ibid.*, 12, 1901, 409-457; 13, 1902, 219-268.

[7] K. Fodor and L. Happisch, Über die Verschiedenheit der Unterschiedsschwellen für den Geschmacksinn bei Reizzunahme, *Arch. f. d. ges. Physiol.*, 197, 1922, 337-347; Die Bedeutung der Zeit zwischen zwei Vergleichsreizen bei Bestimmung von Unterschiedsschwellen, Untersuchungen am Gesichtsorgan, *ibid.*, 201, 1923, 369-375.

[8] W. Köhler, Zur Theorie des Sukzessivvergleichs und der Zeitfehler, *Psychol. Forsch.*, 4, 1923, 137-149.

[9] O. Lauenstein, Ansatz zu einer physiologischen Theorie des Vergleichs und der Zeitfehler, *ibid.*, 17, 1932, 130-177.

[10] C. C. Pratt, The time-error in psychophysical judgments, this JOURNAL, 45, 1933, 292-297.

[11] H. Woodrow, Weight discrimination with a varying standard, this JOURNAL, 45, 1933, 391-416.

[12] J. G. Needham, The time-error as a function of continued experimentation, this JOURNAL, 46, 1934, 558-567.

mentation. The results of these studies indicated the need for modification of Köhler's theory or the adoption of some new theory. Theoretical suggestions for explaining the special facts reported have been proposed by some of these investigators, but none of the hypotheses so far proposed can be considered adequate to all the facts.[13]

It would seem that an important preliminary to the development of a physiological theory of time-error would be the investigation of the neurological levels at which the phenomena responsible for these errors may originate. The type of mechanism or system that may be proposed in explanation naturally depends upon the particular part of the nervous system involved. No one of the investigations thus far made, however, provides direct evidence on this question, which acquires additional interest under the alleged implication of memory. Thus Köhler has suggested that, since the same after-effects of a stimulus may be responsible for the time-errors and for the phenomena of memory, the study of time-error may provide a technique for acquiring information about the early history of the 'memory-trace.'[14] Now since it is likely that the 'memorial' after-effects of stimulation have their locus in the cortex, it becomes important to determine whether evidence may be found that the processes underlying the time-errors are similarly placed.

Three different factors, found at different neurological levels, may be suggested for consideration: (1) after-effects of stimulation left in receptors or nerve-pathways; (2) *reflex* adaptive changes in sense organs (*e.g.* light or pupillary reflex in vision); and (3) persisting changes in the cerebral cortex (*e.g.* the alleged physiological trace).

Our specific problems were to determine, first, whether time-errors occur under our conditions, and if they do what their nature is when conditions are so arranged that they cannot be due to stimulus-after-effects in receptor or in nerve-pathway leading to the brain; and, secondly, how the time-error curve is modified when conditions are so altered that after-effects localized in nerve-pathway may play a rôle in the production of these errors.

These problems may be most conveniently investigated in the field of vision. Impulses from the left and right halves of the retinas are known to reach the visual projection areas of the brain-cortex by separate pathways. The left half of each retina sends its nerve-volleys to the left half of the cortex; the right half of the retina sends its volleys to the right half of

[13] A critical review of theories of the time errors is given by J. G. Needham, The time-error in comparison judgments, *Psychol. Bull.*, 31, 1934, 229-243.

[14] W. Köhler, Zur Theorie des Sukzessivvergleichs und der Zeitfehler, *Psychol. Forsch.*, 4, 1923, 172-175; *Gestalt Psychology*, 1929, 273.

the cortex.[15] Therefore, if the successive stimuli are made to excite *opposite* sides of the retina, the corresponding nerve-impulses will be conducted to the cerebral cortex over separate routes. Under these conditions, after-effects which may occur in the pathways activated by the first stimulus will not be capable of influencing the magnitude of the neural volleys transmitted over pathways activated by the second stimulus. Consequently, any time-errors which occur must depend on effects produced by the first impulse-train on reflex centers in the mid-brain or on mechanisms in the cortex, conditions which are in turn effective in the second stimulation.

Method and Procedure

The stimulus-objects in the experiment consisted of circular areas projected in succession on opposite sides of a fixation-point upon a uniformly illuminated screen. The total series of stimulus-pairs was so constructed that for each time-interval used there would be an equal number of rising and falling steps of a given size in addition to a certain number of pairs in which both stimuli were of equal intensity. The time-errors could then be measured in terms of the difference in percentage of reports of rising and falling intensity at each time-interval. Series of stimulus-pairs in the two different space-orders were alternated and the results of both so combined that the influence of the space-order of the stimuli and of possible differences in sensitivity of the two retinal areas stimulated by a stimulus-pair might be eliminated from the results.

The investigation was made up of two parts. In the first part a group of nine *S*s observed simultaneously. In the second part, the experiment was repeated with a new group of three *S*s. These subjects observed separately and special precautions were taken to prevent eye- or head-movements from affecting the results.

Part I

(A) Stimulus series. The basic unit in the total series consisted of a sequence of 10 stimulus-pairs, the nature of which is indicated in Table I. This series was presented in turn for each of the time-intervals used; *i.e.* 1½, 5, 10, 20 and 30 sec. Each *S* made 50 comparisons in a single experimental hour. At a second experimental period one week later the series was repeated, but with the spatial position of standard and variable reversed as indicated in the table. In the third experimental hour (Period 3) the experiment was repeated with both standard and variable presented in the same spatial position.

As is apparent from the table, the experimental series is so constructed that the spatial and intensive characteristics possessed by stimulus No. 1 (first stimulus)

[15] A. T. Rasmussen, *The Principal Nervous Pathways*, 1932, 28-31. C. J. Herrick, *An Introduction to Neurology*, (4th ed.), 1927, 245 f.

in trials 1-5 will be possessed by the second stimulus in trials 6-10. Consequently, if the results of both sets of trials are combined, the predominance of judgments of a given sort must be entirely attributed to the temporal order of the successive stimulus-objects and cannot be attributed to the other conditions under which the stimuli are presented.

Thus, during trials 1-5 (Period 1) stimulus No. 1, appearing on the left, is provided by Lantern A, and is the variable stimulus. In trials 6-10 these same characteristics are possessed by stimulus No. 2. In Period 2, during trials 1-5, stimulus No. 1 (on right) is provided by Lantern A, and is the variable stimulus. In trials

TABLE I

STIMULUS-PAIRS USED AT EACH TIME-INTERVAL AND ASSOCIATED CONDITIONS UNDER WHICH THE INDIVIDUAL STIMULI WERE PRESENTED

		Trial number			
		1–5		6–10	
Time-order of stimulus-objects		1	2	1	2
Space order of stimulus-objects*	Period 1	L	R	R	L
	Period 2	R	L	L	R
	Period 3	R	R	R	R
Sequence of stimulus-pairs†		170	170	170	170
		152	170	170	152
		176	170	170	176
		164	170	170	164
		188	170	170	188
Lantern used for stimulus at each temporal position		A	B	B	A

* L = left, R = right.

† The figures given to represent the stimulus-pairs refer to the angular opening (degrees) of the episcotister used to control the brightness of the stimulus-objects.

6-10 these same characteristics are possessed by stimulus No. 2. In Period 3, during trials 1-5, stimulus No. 1 (on right) is provided by Lantern A, and is the variable stimulus. In trials 6-10, these same characteristics are possessed by stimulus No. 2.

(B) Spatial relations. The white projection screen used was 94 x 120 cm. with a black circle 1 cm. in diam. at the center to serve as a fixation-point. Extending out a distance of 50 cm. from the screen on either side, at an angle of 135°, were white curtains, intended to provide a relatively uniform field and to block off surrounding objects. The *Ss* sat in two rows, facing the screen, 5 in the rear row and 4 in the front. All the *Ss* had a clear view of the screen and were distributed symmetrically about the plane perpendicular to the center of the screen. The distance from the screen to the plane through the eyes of the nearer *Ss* was 300 cm.; from the screen to the plane through the eyes of the rear row of *Ss*, 310 cm. The diameter of the stimulus-spots was 7 cm. The separation between spots was 8 cm. (4 cm. from the fixation-point to the near edge of the spot). The visual angle subtended by the spots was approximately 1° 20′, with the most central border of the image lying at a visual angle of approximately 46′ from the fixation-point. The angular differences

for the *Ss* may be considered negligible at the distances at which they sat from the screen.

(C) Physical intensities. Before each series of trials with a changed time-interval, a stimulus-pair with both members of equal intensity was presented, in order to indicate the length of the time-interval. Before each change in space-order, the *Ss* were informed of the space-order to be used for first and second stimuli. The 9 *Ss* used in this part of the experiment were all members of the laboratory course in psychology but without previous experience in successive comparisons. They were unaware of the purpose of the experiment.

The stimulus-spots were projected on a white screen with an illumination of 1.1 meter-candles. The brightness of the standard stimulus-spot, determined by an opening of 170° in the episcotister in front of the projection-lanterns, corresponded to an illumination of 13.8 meter-candles. Assuming a reflection coefficient of about 0.8 for the white screen, the corresponding brightnesses of the two regions were 0.09 millilamberts for the screen, and 1.1 millilamberts for the standard stimulus-spot. Ten minutes were allowed for adaptation of *S* to the brightness of the screen and experimental room before the beginning of observation.

(D) Durations. The duration of each stimulus object was 0.6 sec. The interval between the end of stimulus No. 1 and beginning of stimulus No. 2 ranged from 1.5 sec. to 30 sec. as already indicated. The interval between stimulus pairs was 1 min. The interval between the ready-signal and stimulus No. 1 was 2 sec.

(E) Instructions to subjects. *S* was instructed (1) to look steadily (upon the ready-signal) at the fixation-point (both eyes) where two circular areas would appear in succession (one on the left side and one on the right) and (2) to report whether the second circle appeared brighter, less bright than, or equal in brightness to, the first. *S* was cautioned to maintain fixation until the second stimulus-circle had disappeared. The reports were recorded by the *Ss* upon narrow slips of paper ruled off to correspond to the 50 stimulus-pairs to be presented: '+' for second-stimulus-brighter, '—' for second-stimulus-less-bright, '=' for second-stimulus-equal-in-brightness, '?' for doubt-upon-relative-intensity. When *S*'s fixation so shifted that he looked directly at the stimulus-spot the record was 'doubtful.' After each report the place marked was folded under in order to eliminate the possible influence of knowledge of earlier upon later reports. *S* was warned that it was not desirable between pairs to maintain the fixation on the center-point, but to close the eyes momentarily or look about within the region limited by the ends of the white curtains. Excessive fatigue of the eyes was thus avoided. In Period 3, *S* was informed that the successive stimulus-circles would appear *at the same place* on the screen.

(F) Apparatus. Illumination of the screen was provided by a 100-watt frosted lamp behind and above the heads of the *Ss* and lying in the plane perpendicular to the screen at its center. The areas used as stimulus-objects were projected on the screen from two identical projection-lanterns (A and B) placed one on each side of the general illumination-lamp. A thin metal plate with a small hole bored in the center was used as a lantern-slide for each lantern. To control exposure-time and the intervals between successive stimuli, a light lever-arm (serving as a shutter) was set in front of the objective of each lantern. These shutters were controlled by electromagnets connected in series with contacts around the graduated arc of a Lewin time-sense apparatus. A reversing switch inserted in series between the contacts and the electromagnets of the two shutters permitted control of the

temporal order in which the shutters were operated. Thus one position of the reversing switch resulted in the shutter of Lantern A being actuated first; the opposite position of the switch operated first the shutter of Lantern B.

The physical brightness of the stimulus-circles was controlled by means of episcotisters placed in front of the shutters. An opening of 170° in the episcotister in front of the right-hand lantern (B) was used to provide the standard stimulus; openings of 152°, 164°, 170°, 176°, and 188° in the episcotister in front of the left-hand lantern (A) were used to provide the variations in brightness of the variable stimulus.

Both lanterns were mounted on movable platforms pivoted at the back end to permit control of the direction of the lantern relative to the central plane. By such adjustments of direction the positions of the images on the screen could be regulated. The angular directions of the two lanterns relative to the central plane were always made equal and opposite so that differences in the characteristics of the two stimulus-circles would not be produced by differences in the direction of the optical axes of the two projection-lanterns. As already indicated, identical stimulus-series were used in periods one and two, except for a reversal of space order. This difference in space-order in the two periods was brought about by means of slight reciprocal adjustments in the directions of the two lanterns, so that the left-hand stimulus was provided by Lantern A in the first period, and by Lantern B in the second.

Part II

The conditions here were like those in Part I except in the particulars noted below. The aim was to impose more rigid limitations on visual-fixation in order that comparisons could not occur except on the basis of successive stimuli impinging on opposite sides of the retina. Each *S* observed by himself and was seated with his right eye in the plane perpendicular to the screen at its center. The left eye was covered by a black eye-patch. A light metal framework containing two opaque slides was fixed closely over the right eye and held in place by strings tied around the head. These slides moved horizontally and permitted the width of the opening through which *S* looked to be so adjusted that the stimulus-circles could be seen in indirect vision when the gaze was fixed on the fixation-point, but could not be seen if the eye moved to look more directly at the stimulus-object. The adjustment of the slides was made by *S* himself at the beginning of the experimental period and the slides remained fixed in the same position for the entire experimental series. This device made it impossible for comparisons to be made except when the successive stimuli excited opposite sides of the retina so long as the position of the head did not change.

For immobilizing the head a device was used which held the head rigidly in front and on two sides. *S* first placed his forehead against a horizontal cross-bar: from the sides two horizontal side-bars could be screwed in until they touched the sides of the head. The pressure of the side-bars was then gradually increased until the head was held rigidly but without discomfort. This arrangement prevented movements frontward, backward, or from side to side. In this part of the experiment, *S* made oral reports to *E*. In case both members of a stimulus-pair were not seen (as a consequence of eye-movements) this pair was immediately repeated. Such a repetition was, however, very rare.

The distance of *S*'s eye from the screen was 117 cm. The distance from the

center of the fixation-point to the nearer edge of each stimulus-spot was 46.7 cm. The corresponding visual angle was 21° 45′. The visual angle subtended by the diameter of the stimulus-spot was 2° 52′. These distances were so selected that images of the stimulus-objects would fall on the retina well outside the foveal region. Preliminary tests in which the place of the blind-spot was determined made sure

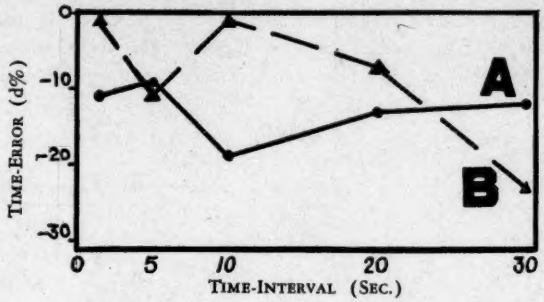

FIG. 1. TIME-ERROR AS A FUNCTION OF TIME-INTERVAL (PART I)
(A, successive stimuli fall on opposite sides of retina; B, successive stimuli fall on same retinal area.)

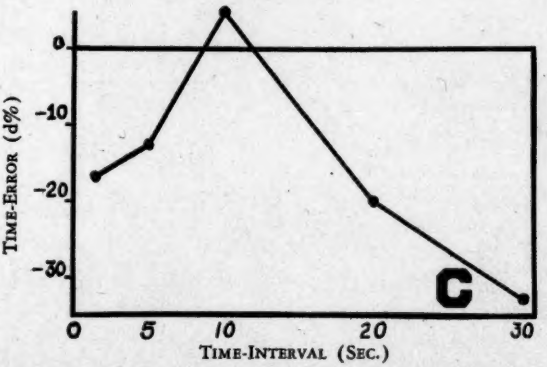

FIG. 2. TIME-ERROR AS A FUNCTION OF TIME-INTERVAL (PART II)
(Successive stimuli fall on opposite sides of retina.)

that the images did not fall within the blind-spot. Three new *Ss* were used. They were selected on the basis of previous experience in experiments requiring steady fixation. They were unaware of the purpose of the present study.

RESULTS

The results of the study are summarized in Figs. 1 and 2. The curves here represent dependence of the time-errors on the intervals of time

between the successive stimuli. They may be designated as time-error curves, or, in Needham's terminology, as *p-functions* (*p* referring to the pause between successive stimuli). Curve A shows the time-errors obtained when successive stimulus-objects were presented on opposite sides of the fixation-point (Part I, Periods 1 and 2). Curve B shows the time-errors obtained when successive stimulus-objects were presented in the same position on the right side of the fixation-point (Period 3). In each case the time-error for each time-interval was determined by subtracting the

TABLE II

RESULTS OF EXPERIMENTS OF PARTS I AND II

Retinal Areas Stimulated	No. of *Ss*	Report on 2d stimulus-object	Reports at various time-intervals (sec.)				
			1.5	5	10	20	30
(A) Opposite sides of retina (Part I)	9	Brighter (B)	76	71	77	74	69
		Equal	48	54	61	56	64
		Darker (D)	56	55	42	50	47
		Total (N)	180	180	180	180	180
		d%*	−11	− 9	−19	−13	−12
(B) Same retinal area (Part I)	9	Brighter	31	33	34	34	41
		Equal	27	34	23	28	29
		Darker	32	23	33	28	20
		Total	90	90	90	90	90
		d%*	− 1	−10	− 1	− 6	−21
(C) Opposite sides of retina (Part II)	3	Brighter	28	27	22	29	36
		Equal	14	14	13	14	8
		Darker	18	19	25	17	16
		Total	60	60	60	60	60
		d%*	−17	−13	+ 5	−20	−33

*d%=(D−B)/N (in percentage).

number of reports of rising intensity (second stimulus reported brighter) from the number of judgments of falling intensity (second stimulus reported less bright or darker), and expressing this difference as a percentage of the total number of comparison-pairs for that time-interval. In making these calculations Köhler's practice of combining the results of all *Ss* in the group was followed. Curve C (Fig. 2) shows the time error-curve obtained in Part II, when measures were taken to limit the possible influence of head and eye-movements on the results. It represents the combined results for the three new *Ss*. The data on which the various curves are based are given in Table II. Under the caption 'equal' the reports of both 'equal' and 'doubtful' were combined.

The facts indicated by the curves may be summarized as follows:

(1) Curve A (Fig. 1): Time-error as a function of time-interval when successive stimulus-objects appear on opposite sides of fixation-point. Negative time-errors occur for all time-intervals ranging from 1.5 to 30 sec. The time-error curve is substantially flat, that is to say, there is no apparent general increase in absolute magnitude of negative time-error with increase in time-interval.

(2) Curve B (Fig. 1): Time-error as a function of time-interval when successive stimulus-objects are presented in the same place. The difference in Curve B compared with Curve A consists in a slight increase in the negative slope of the curve when regarded as a whole. There is a rise in the level of the curve from 5 to 10 sec. and a fall over the larger intervals, *i.e.* 10 to 30 sec.

(3) Curve C (Fig. 2): Time-error as a function of time-interval when successive stimuli appear on opposite sides of the fixation-point (special precautions taken to eliminate influence of eye- or head-movements). Curve C shows, as does Curve A, that the time-errors occurring are predominantly negative. Only at the 10-second interval does a small positive error occur. One respect in which this curve differs from Curve A consists in the marked negative slope shown after intervals of 10 sec.; in other words, the phenomenon of increase of absolute magnitude of negative time-error with time-interval is observable.

Discussion

The following sections are limited to a discussion of the neurological locus of the factors underlying the time-errors exhibited in the present study. No attempt is made to formulate a comprehensive theory adequate to explain all the reports on the conditions influencing time-error so far made in the literature.

(A) Time-error factors operative when successive stimuli impinge on opposite sides of the retina. Curves A and C demonstrate that negative time-errors may occur when the influence of possible after-effects of excitation produced in neural receptors or pathways is eliminated, for the experimental conditions under which these curves were obtained involved the excitation of different retinal areas and the conduction of the excitations to the brain over separate routes. It seems necessary to conclude, therefore, that brain mechanisms are primarily responsible for the negative time-errors found under these conditions.

In explanation of our time-error curves, two types of mechanism must be considered: (I) a pupillary or light reflex which is dependent on reflex connections in the midbrain, and (II) cortical phenomena underlying the process of successive comparison.

(1) Light reflex mechanism. The term 'pupillary' or 'light' reflex refers to the modifications in the size of the pupil which occur when the intensity of light entering the eye changes, the diameter of the pupil varying inversely with the intensity of the light. The diameter of the pupil at a given moment is a function of opposing tensions produced by the sphincter and the dilator muscles of the iris.

The first stage of the reflex arcs regulating the action of these muscles consists of connections between the retina and nerve-cells in the superior colliculus. Beyond this stage the pathways regulating the action of the two muscles are separate. The pathway to the sphincter muscle is from the superior colliculus to the nucleus of the third cranial nerve, then to the ciliary ganglion, and finally to the sphincter muscle of the iris. The pathway to the dilator muscle is from the superior colliculus to Budge's ciliospinal center in the upper thoracic segments of the spinal cord, then to the superior cervical ganglion, and finally to the dilator muscle of the iris.[16]

Stimulation of either side of the retina by the initial stimulus of a comparison-pair may be expected to bring about a decrease in the size of the pupil followed by a gradual return to its original size. If the pupil were still contracted when the second stimulus occurred, a diminished amount of light would enter the eye. A second stimulus of like energy with the first would therefore tend to be judged as less intense than the first. The contracted state of the pupil would thus lead to the production of positive time-errors, the magnitude of which would vary inversely with the size of the pupil.

It is apparent that the resultant negative time errors exhibited by Curves A and C cannot be attributed to the operation of the light reflex, for this reflex could have been responsible only for the production of positive time-errors. In case such a positive time-error component were involved under the conditions of the present experiment, it would mean that the negative time-error component would have to be regarded as still greater than that represented in our curves, in order to compensate for the positive time-error component.

The question may now be raised whether the light reflex actually was a factor tending to the production of positive time-errors. That is to say, at time-intervals as long as 1.5 sec. after the initial stimulus (the shortest interval used in the present experiment) must it be supposed that the pupil had not yet returned to its original size. The most complete data I have found on the rate at which the pupil returns to its original size after visual stimulation are those of Reeves.[17]

[16] A. T. Rasmussen, *op. cit.*, 32-35; C. J. Herrick, *op. cit.*, 246.

[17] P. Reeves, Rate of pupillary dilation and contraction, *Psychol. Rev.*, 25, 1918, 330-340.

If the determinations of this author are taken as directly applicable to the present study, then calculations of the sort summarized below indicate that at 1.5 sec. after an initial stimulus, a decrease in diameter of the pupil equal to 3.8% of its initial diameter may still be present; and that a difference greater than 1% (the relative difference limen in vision) may be present up to intervals of about 25 sec. On the basis of these results, it would be necessary to regard the pupillary reflex as a possible factor in the production of positive time-errors up to intervals of about 25 sec. Certain of the ways in which the conditions of the present experiment differ from those of Reeves suggests, however, that the decrease in diameter of the pupil would not be as great as suggested by the calculations just mentioned. The significance of still other differences in the two sets of conditions is difficult to estimate. It seems necessary to conclude, therefore, that the pupillary reflex may possibly be a factor in the production of positive time-errors in the present experiment up to time-intervals of about 25 sec. following stimulation, but that a reliable estimate of the extent to which it is actually a factor must await the performance of experiments in which changes in pupillary diameter are measured under the conditions of the present experiments. The calculations outlined indicate, moreover, that in previous studies upon the time-errors that have been performed in the field of vision, the pupillary reflex must be definitely taken into account as a possible factor.

The essential steps involved in the calculations based on Reeves' data are as follows:

(i) Our initial problem is to determine the magnitude of the decrease in diameter of the pupil remaining 1.5 sec. after the initial stimulus of a comparison-pair has been presented.

(ii) The data presented by Reeves which permit an approximate calculation of this magnitude consist of tables and curves representing the following relationships: (1) the diameter of the pupil at various fixed brightnesses of the visual field (cf. his Table II); (2) the rate of closing of the pupil upon transition from a field brightness of one millionth of a millilambert to a brightness of one hundred millilamberts (Table III); and (3) the rate of opening of the pupil upon change from a field brightness of one hundred millilamberts to a brightness of one millionth of a millilambert.

Calculations based on Reeves' tables show that in the experiment on the closing of the pupil, in 0.6 sec. (the duration of the stimulus in the present experiment), 35% of the total decrease in size has occurred. In the experiments on opening of the pupil, in 1.5 sec. (the duration of our shortest interval after a stimulus), 33% of the total increase in diameter has occurred; that is to say, 77% of the total increase in size remains to be attained. If it is assumed, for purposes of calculation, that contraction and expansion of the pupil will occur at the same rate under the conditions of the present study as in Reeves' experiments, the changes in pupillary size to be expected can be estimated.

(iii) Consider first the *decrease* in size of the pupil to be expected as a result of the action of the stimulus. A change in field brightness from 0.09 millilamberts to 1.6 millilamberts would lead to a change in pupillary diameter from 6.1 mm. to 5.0 mm., a change of 1.1 mm.—provided the eye has a sufficiently long time to adjust to the reduced brightness (Reeves' Table II). In 0.6 sec., however, the fractional change would be only 35% of the total change, or 0.314 mm.

(iv) Consider now the extent to which the pupil will *increase* in size when the light stimulus is removed. If 0.314 mm. (calculated above) is taken to represent the increase in diameter that would take place if a sufficiently long interval elapsed following stimulation, then Reeves' data indicate that in 1.5 sec. 77% of this total change would not yet have been attained. The difference in pupillary size from the original diameter of 6.1 mm. would thus be 0.77 multiplied by 0.314 or 0.24 mm.

This difference in pupillary diameter is 3.8% of the initial diameter of 6.1 mm. If it could be assumed that this decrease in diameter actually remained 1.5 sec. after the stimulus had been presented, it would be necessary to conclude that the light entering the eye upon the presentation of a second stimulus would be reduced by 3.8%. This difference is well above the commonly accepted value for the relative difference limen of 1%.[18] Further calculations based on Reeves' data show that an interval of 25 sec. must elapse before the difference in the pupillary diameter would fall to a value not greater than 1% of the initial diameter. On the basis of such calculations, one might conclude that up to intervals of about 25 sec., the contraction in the size of the pupil might be a factor in the production of positive time-errors.

There are a number of reasons for supposing, however, that the decrease in diameter of the pupil under the conditions of the present experiments would not be as great as suggested by the above line of reasoning. Reeves' data on the diameter of pupil corresponding to different brightnesses of the visual field were obtained with the entire visual field illuminated. In the present experiment, on the contrary, the stimulus did not consist of an increased illumination of the entire visual field, but only of a relatively small area in the periphery. Reports in the literature show that the contraction of the pupil produced by an increase in illumination is markedly reduced as the location of the stimulated area departs further and further from the fovea and that the pupillary contraction is less marked, the smaller the retinal area stimulated.[19]

The significance of still other differences in the conditions of Reeves' experiment and the present one are more difficult to estimate. In Reeves' determination of the rate of opening and closing of the pupil, the total changes produced correspond to extremely large differences in the brightness of the field. It is not likely that the *rate of change* will be exactly the same when the total change is so much smaller, as in the present experiments. Finally, in determination of the rate of opening of the pupil, Reeves' *S*s were exposed to the initial brightness for a period long enough to permit the pupil to reach its equilibrium or final diameter. In the case of the present experiment, however, the diameter of the pupil at the end of the 0.6-sec. stimulus represented not a terminal but an intermediate point in contracting. Any definite conclusions concerning the rôle of pupillary contraction in producing positive time-errors in the present experiment must, in view of these uncertainties, await further investigation.

(2) Cortical factors. A second mechanism to be considered as a possible factor in the production of the time-errors exhibited by Curves A and C consists of phenomena localized in the cerebral cortex and involved in the

[18] L. T. Troland, *The Principles of Psychophysiology; Sensation*, 2, 1930, 79.
[19] C. Behr, *Die Lehre von den Pupillenbewegungen*, 1924, 1-221.

process of successive comparison. Köhler's theory of the negative time-error assumes this location of the processes underlying negative time-errors.[20]

The discussion of the previous section has indicated that the light reflex was probably not a factor in the production of the time-errors found in the present study, and that even if it were operative it could have led to the production of positive time-errors only. The predominantly negative time-errors exhibited by Curves A and C must be attributed therefore to other factors. In terms of present knowledge of neurological pathways, and excluding the operation of chance factors, the only other possible factor that could have been involved in the experiments in question would seem to be a mechanism or mechanisms lodged in the cerebral cortex. It may therefore be concluded that the negative time-errors exhibited by Curves A and C depend upon processes having their locus there.

The particular temporal course of the cortical factors which tend to produce negative time-errors cannot be derived from our results. Curve A fails to show the phenomenon of an increased magnitude of negative time-error with increasing time interval; Curve C does exhibit this phenomenon. Which curve more correctly represents the true state of affairs must be determined from further experimentation.

It is of interest that Curve C shows an increase in level up to intervals of 10 sec., after which a decided decrease in level occurs. The early rise in level might be accounted for either in terms of a decrease in the absolute magnitude of a negative time-error component or an increase in the magnitude of a positive time-error component. The fact that Curve C rises above the X-axis at the 10 sec.-interval makes it more plausible to attribute the change to an increase in a positive component. Moreover, if the negative time-error factor in the cortex is regarded as dependent on the sinking with time of the "intensive property" of the brain-trace representing the initial stimulus-object, as is done in Köhler's theory, it is necessary to assume a separate *positive* time-error factor to account for the rise in the level of the curve for intervals up to 10 sec. There is thus an indication in this phenomenon of rise of level of the early parts of the curve that the cortex is the locus not only of a negative time-error factor, as suggested by the previous discussion, but also of a positive time-error factor. In view of the differences in form between Curves A and C, however, not too much reliance should be placed on the specific form of Curve C, without more extensive experiments on additional groups of *Ss*.

(B) Time-error factors in neural pathways or receptors. Curve B represents the time-error curve obtained with the original group of subjects when the successive stimuli impinged on the same peripheral retinal area. When this curve is compared with Curve A evidence is found of some

[20] W. Köhler, Zur Theorie des Sukzessivvergleichs und der Zeitfehler, *Psychol. Forsch.*, 4, 1923, 134-146.

increase in height of the early parts of the curve, associated with an increase in the negative slope of the curve. If these changes in the curve are regarded as due to the fact that the successive stimuli now impinge on identical retinal areas rather than on areas on opposite sides of the retina, then it must be supposed that additional factors in the production of time-errors are localized in neural pathways to the brain. The curve actually obtained must consequently be regarded as a resultant of the combined effect of both sets of factors, the cortical factors tending to produce negative time-errors and the time-error factors localized in neural pathways.

It is worthy of note that the rapid fall in level shown by Curve B after intervals of 10 sec. is also found in Curve C. The latter curve was obtained on different *Ss*, and with the successive stimuli impinging on different retinal areas, rather than on the same retinal area, as was the case for Curve B. Whether any significance is to be attributed to this similarity cannot be definitely stated on the basis of present data. The presence of this phenomenon in both curves raises the question of whether the fall in level of Curve B beyond intervals of 10 sec. may not be due to central, rather than to peripheral factors, as suggested above. A decisive answer to this question can be given only after further experiments are conducted with additional groups of *Ss* to determine whether the changes exhibited by Curve B relative to Curve A may be reliably attributed to the change in retinal areas effected by the successive stimuli. Provisionally, the changes shown by Curve B relative to Curve A may be regarded as due to the fact that the successive stimuli of a pair now impinge on the same retinal area, and we may inquire concerning the possible physiological basis for these effects.

After-effects of stimulation left behind in receptors or neural pathways might be of two kinds: excitatory after-effects which would increase the effectiveness of the second stimulus and lead to a tendency for a second like stimulus-object to be seen as brighter than the first; or inhibitory after-effects, which would decrease the effectiveness of the second stimulus and lead to a tendency for the second stimulus-object to be judged less bright than the first. Excitatory after-effects would thus tend to produce negative time-errors, while inhibitory ones would tend to produce positive time-errors.

The increased negative slope which Curve B shows relative to Curve A may be explained in either one of two ways; first, by supposing that there has been added to Curve A a negative time-error component which *increases* in absolute magnitude with time-interval (that is to say, a negative time-error curve of negative slope); or, secondly, by supposing that there has been added to Curve A a positive time-error component which *decreases* in absolute magnitude with time-interval (that is to say, a positive time-error curve of negative slope). If a *negative* time-error component had been added at the early time-intervals to an already existing negative time-

error, then the level of the early parts of the curve would be correspondingly lower, since both time-error factors would have the same sign. On the other hand, if a *positive* time-error component were added in the early parts of the curve to an already existent negative time-error, the level of the early parts of the curve would be raised. Since Curve B shows a higher level for the early parts of the curve as well as an increase in negative slope, it seems plausible to attribute the change to the introduction of a positive time-error component. The fact that the resultant curve lies wholly below the X-axis may be explained by assuming that the negative time-error component due to cortical factors is greater in absolute magnitude than the positive time-error component contributed by the pathways at any of the time-intervals used.

The conclusion that the contribution of the pathways is a positive time-error component is further supported when we consider the nature of the factors in the pathways which would produce negative or positive time-errors, respectively. To account for the provision of a negative time-error component by the pathways, excitatory after-effects in the pathways must be assumed, as indicated above; but to proceed further and to account for the increased negative slope in terms of changes in the excitatory after-effect, it is necessary to assume that this after-effect increases in magnitude with time and thus would lead to greater and greater negative time-errors. Though it is plausible to suppose that an excitatory after-effect would require some time to reach its maximal level, and so might show a slight period of increase in its earliest stages, it seems very unlikely that an excitatory process following stimulation could continue to build up for periods as long as 30 sec. Such an assumption would be required to account for the increased negative slope exhibited by Curve B.

If, on the other hand, we suppose that the pathways contribute a positive time-error component, then we must attribute such a component to the occurrence of an inhibitory after-effect following stimulation, that is to say, to a reduced level of excitability in the pathways. To account now for the increase in the negative slope of the curve, it is necessary only to suppose that this inhibitory after-effect, or state of decreased excitability, gradually disappears with time. The assumption of a gradual disappearance of any given after-affect with time would seem necessary in any case. It may accordingly be concluded that the result of repeated activity of a particular neural pathway is to contribute a positive time-error component to the resultant time-error curve, and that this positive time-error component is most plausibly accounted for on the basis of a reduction in excitability at some region of the neural pathway following the initial stimulus.

We may now inquire concerning the particular region of the visual pathways at which such a time-error factor most probably exists. The successive neural stages involved in the transmission of impulses from the cones and rods of the retina to the cortex consists of three stages of neurones represented in the retina itself (rods and cones, bipolar cells, and the ganglion cells which give rise to the fibers of the optic nerve) and a final neurone-stage which arises in the lateral geniculate body and possibly also in the pulvinar nucleus and terminates in the projection-areas of the cortex.[21] Involved in this pathway to the cortex are three types of tissues; receptor cells of the retina, axons, and synapses between successive neurones.

As to the region or part of the visual pathway at which inhibitory or refractory after-effects might exist, three possibilities must therefore be considered; (a) a decrease in sensitivity or excitability of the receptor cells; (b) a decrease in excitability of nerve-fibers or axons; and (c) a decrease in excitability at the synapses, as represented, *e.g.* by an increase in the so-called inhibitory state in the synapse.

The literature concerning these different types of tissue fails to provide an adequate basis for a reliable inference concerning the particular tissue in which the inhibitory after-effects indicated by our results have their locus. It is possible, however, to exclude quite definitely as a factor the relative refractory phase of the nerve-fiber. This period is terminated in less than 8 millisec. in the slowest optic nerve fibers, and thus is completed in intervals of time much shorter than those involved in this investigation. There is some evidence, on the other hand, that receptor cells and synapses may show depressions of excitability following stimulation which may last considerably longer than those characterizing the axon. The data are not sufficient, however, for precise calculations of the length of time such after-effects might last in the visual system, or whether they might last long enough to account for the positive time-error component indicated by Curve C. It may be provisionally concluded, therefore, that either receptor cells or synaptic junctions might be the locus for the inhibitory after-effects suggested by our results. More definite conclusions concerning these possibilities must be left to future investigations.

The evidence concerning after-effects of stimulation in nerve-fibers, receptor cells, and synapses may be briefly reviewed.

(i) Refractory period of optic nerve-fibers. Bishop[22] reports that the optic nerve of the rabbit, which may be considered representative of the mammalian type of optic

[21] A. T. Rasmussen, *The Principal Nervous Pathways*, 1932, 34-35; C. J. Herrick, *An Introduction to Neurology*, (4th ed.), 1927, 240-248.

[22] G. H. Bishop, Fiber groups in the optic nerve, *Amer. J. Physiol.*, 106, 1933, 460-474.

nerve, is made up of two groups of fibers, differentiated on the basis of the rate of conduction of the action potential. Homologous groups are found in the optic nerve of the frog, and in addition, a third, still more slowly conducting, group, not found in the rabbit nerve. Determinations of refractory period are reported for the frog nerve. The values given are presumably for the absolute refractory period. The largest refractory period reported for the action potential of the second group of fibers, which correspond to the slower fibers of the rabbit nerve, is 1.62 millisec.[23] If it is assumed that the total refractory period (absolute plus relative) lasts about 4.8 times the absolute refractory period, then the total refractory period of the frog axon would be 7.8 millisec. The duration of the total refractory period of the corresponding *mammalian* nerve-fiber would be even less. This value is considerably smaller than the minimal duration of 1.5 sec. used in the present study. The refractory period of the optic nerve fiber may therefore be excluded as a factor in the positive time-errors found in this study.

The value of 4.8 taken above to represent the ratio of total to absolute refractory period is based on the curve given by Adrian and Lucas to represent the rate of recovery of the frog nerve from stimulation.[24] Data given by other workers indicate ratios of the same order of magnitude though differing slightly in absolute value. Thus Forbes[25] gives values of 2 millisec. for the absolute and 8 millisec. for the total refractory periods of medullated frog nerve; these data provide accordingly a ratio of 4.

(ii) Receptor cells. In a search for evidence concerning the after-effects of stimulation of visual receptor cells, there are a number of types of investigation to which one might turn. These include (1) the investigation of after-images; (2) investigations of retinal potentials; (3) investigations of action potentials of the optic nerve; and (4) investigations of related properties of the tissue which might provide a basis for calculation of the duration of a recovery-period following stimulation.

The phenomena of *after-images* are quite generally attributed to processes in the receptors.[26] On this basis one might use the changes in character of the after-image as an index of changes in excitability of the receptor cells concerned. Such an interpretation is limited, however, by the fact that different types of theory have been proposed to explain the series of positive and negative after-images which may follow stimulation.[27] In connection with the specific problem of inferring the changes in excitability of receptors following stimulation under conditions similar to those involved in the present study, there are added difficulties. There is, first of all, an absence of descriptive accounts of the course of after-images appearing under the particular conditions used in the present study. Secondly, there is evidence that the nature and duration of after-images are greatly influenced by the particular stimulus conditions involved.[28] We are hardly in a position, therefore, to estimate the rôle of

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[23] *Ibid.*, 470.

[24] Reproduced in W. M. Bayliss, *Principles of General Physiology*, (4th ed.), 1931, 389.

[25] A. Forbes, The mechanism of reaction, *Handbook of Experimental Psychol.*, Clark University Press, 1934, 108.

[26] J. Fröbes, *Lehrbuch der experimentellen Psychologie*, (3rd ed.), 1923, 82; L. T. Troland, *The Principles of Psychophysiology; Sensation*, 2, 1930, 90-96.

[27] J. Fröbes, *op. cit.*, 78-80.

[28] A. Juhasz, Über die komplementärgefarbten Nachbilder, *Zsch. f. Sinnesphysiol.*, 51, 1920, 233-263.

after-image factors in the present study. About all that can be said is that under certain conditions negative after-images may last for relatively long intervals. Juhasz,[20] for example, has observed them lasting as long as one minute, though a duration as long as this occurs only with a stimulus of relatively long duration. The data on negative after-images thus suggest that a slow rate of recovery of receptors following stimulation may be a factor in the positive time-errors under consideration.

Data on *retinal potentials* have furnished relatively little information concerning processes in receptor cells. These potentials are very complex affairs which seem to depend on many factors. Until the contribution of the various factors is better understood, retinal potentials would seem of little value as an index of changes in excitability of receptor cells following stimulation.[20]

The recording of *action potentials of the optic nerve* represents a third technique that might conceivably furnish evidence concerning the course of changes in excitability of receptor cells. The data obtained by means of this technique are, however, ambiguous when the investigations are carried out upon a mammalian type of eye. The nerve-volleys which are transmitted by the optic nerve of a mammalian eye have already passed through two earlier stages of neurones. The electrical records obtained from such volleys may not, therefore, afford a correct picture of the character of the trains of impulse initiated by the receptor cells. To obtain more direct evidence concerning the nature of the events occurring in receptor cells, certain workers have turned to a simpler type of eye, namely that of limulus, in which the three neurone-stages are lacking, and the optic nerve-fibers proceed directly from the receptor cells.[21] No report has yet been published, however, of the use of this technique in the determination of the changes in excitability of the visual receptor cells following stimulation.

There is, however, evidence concerning other types of receptor which indicates that they require a longer time than the nerve-fiber to recover from stimulation. This evidence relates to the frequencies at which regular rhythms of discharge occur in receptor cells which are acted upon by a constant stimulus. Adrian[22] has pointed out that the interrupted nature of the discharge is due to the refractory period of the tissue and that the minimal and maximal rates of regular discharge are functions of the total and absolute refractory periods, respectively. Thus after a given tissue has discharged as a result of the action of a stimulus, it will not be able to discharge again until it has reached a point in its relative refractory phase at which its irritability is great enough to allow it to respond again under stimulation. With the minimal stimulus capable of producing a response in a rested sense organ, the tissue will not be able to respond again until it has recovered completely, *i.e.* at the end of the total refractory period. The rate of discharge will thus be the reciprocal of the total refractory period. If a maximal stimulus is used, a second discharge following

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[20] *Ibid.*

[20] For a discussion of possible factors in retinal potentials, see G. H. Bishop, Electrical responses accompanying activity of the optic pathway, *Arch. Ophthamol.*, 14, 1935, 997-1004.

[21] H. K. Hartline and C. H. Graham, Nerve impulses from single receptors in the eye, *J. Cell & Comp. Physiol.*, 1, 1932, 277-295; C. H. Graham and H. K. Hartline, The response of single visual sense cells to lights of different wave lengths, *J. Gen. Physiol.*, 18, 1935, 917-931.

[22] E. D. Adrian, *The Mechanism of Nervous Action*, 1932, 27-29. See also D. W. Bronk, The activity of nerve cells, *Cold Spring Harbor Symposia on Quantitative Biology*, (IV), 1936, 171.

an earlier one may occur as soon as the absolute refractory period is terminated. The maximal repetitive frequency possible for the tissue will therefore be the reciprocal of the absolute refractory period. Accordingly, the duration of total and absolute refractory periods may be determined from the minimal and maximal rates respectively at which regular frequencies of discharge will occur.

As Adrian points out, such sense organs as muscle-spindle and pressure receptors may discharge regularly at frequencies as low as 5 or 10 a second.[33] These frequencies correspond to a total refractory period as long as 1/5-sec. and 1/10-sec., respectively, durations much longer than the total refractory period of nerve fibers.

If it were assumed that the total refractory period of visual receptor cells is not still greater than the 1/5-sec. indicated above for muscle-spindles, then this period would not be great enough to account for the positive time-errors found in the present study. Such a conclusion would exclude the receptor cells as a factor in the positive time-errors reported here and would suggest the synaptic junction as the most probable locus of the inhibitory after-effects presumed to appear in the pathways following activity.

A fourth method that might conceivably be useful in determination of the rate of recovery from stimulation of visual receptor cells is that based on determination of related properties of the tissue. In connection with certain tissues, numerous workers have contributed evidence of inter-relations among different properties.[34] It has been reported, for example, that nerve-fibers of relatively long refractory periods tend to show as associated properties action-potential spikes of long duration, relatively slow velocity of propagation of the nerve-impulse, and long chronaxies. Similar relations have been reported for muscle-fibers. The assumption of like relations for visual receptor cells would provide additional methods for investigation of the rate of recovery of these cells from stimulation.

(iii) Changes in excitability at the synapses. A third locus in the visual pathways at which the excitability might possibly be reduced following stimulation is in the synapses. In the transmission of neural volleys along the visual pathways from retinal receptor cells to the cerebral cortex, three synaptic junctions occur. States of inhibition following transmission of a nerve impulse might conceivably occur at one or more of these junctions and so lead to a decrease in the 'strength' of the volleys under transmission to the brain.

Evidence that many reflexes have long refractory periods compared to that of the nerve-fiber has been given by a number of investigators. Thus Sherrington has reported a total refractory period for the scratch reflex of about 1/4-sec., and for the "extensor-thrust" of as much as 1-sec.[35] Dodge reports a refractory period for the knee-jerk of about 1/2-sec. and for the lid reflex of 3-sec. or greater.[36]

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[33] E. D. Adrian, *op. cit.*, 27.

[34] R. S. Lillie, *Protoplasmic Action and Nervous Action*, (2nd ed.), 1932, 325-330; H. S. Gasser, Axon action potentials in nerve, *Cold Spring Harbor Symposia on Quantitative Biology*, (I), 1933, 140; E. A. Blair and J. Erlanger, A comparison of the characteristics of axons through their individual electrical responses, *Amer. J. Physiol.*, 106, 1933, 524; A. Forbes, The mechanism of reaction, *Handbook of Experimental Psychology*, Clark University Press, 1934, 168 f.; L. Lapicque, *L'excitabilité en fonction du temps*, 1926, 353-358; H. Davis and A. Forbes, Chronaxie, *Physiol. Rev.*, 16, 1936, 407-441.

[35] C. S. Sherrington, *The Integrative Action of the Nervous System*, 1906, 45 f., 49, 68.

[36] R. Dodge, *Human Variability*, 1931, 15. The literature on the refractory period

Sherrington has considered in detail the pathway involved in the scratch-reflex and has given evidence to show that the seat of its long refractory period is in the gray matter of the spinal cord, between the final common path to the muscle, and the receptor-neurone from the skin.[37] He has pointed out that the refractory state is "akin to a state of inhibition," and thus suggests its relation to other reflex phenomena which indicate the existence of central inhibitory states.[38] The synapse is regarded by him as the specific part of the gray matter of the reflex arc which is responsible for the differences of reflex-arc conduction from nerve-trunk conduction.[39]

Granit, and more recently, Graham, have presented evidence to show that many of the phenomena attributed by Sherrington to motor synapses may also be exhibited by synapses occurring in the visual pathways.[40] It seems tenable, therefore, as a working hypothesis to suppose that, following transmission of a nerve-impulse across a synapse in the visual pathway, an increase in the inhibitory state in the synapse occurs and that this inhibitory state results in a reduction in the effective 'strength' of the total series of neural volleys initiated by later stimuli. The long refractory periods found for certain motor reflexes suggest, further, that the inhibitory after-effects in visual synapses might last long enough to account for positive time-errors obtained with time-intervals even as long as those used in the present study. More direct examination of this possibility must be left for further investigations.

A plausible basis for long refractory periods in complex neurological systems is suggested by Dodge. He points out that, if recovery of irritability is an oscillatory process, a relative refractory period may be expected to last until the several phases of complete recovery coincide in all the links of the chain.[41] The visual pathway from receptor cells to cortex involves, as we have seen, three synaptic junctions. Differences in the rhythms of excitability at these different junctions would provide a basis for a considerable prolongation of the relative refractory period to nerve-impulses transmitted over the entire pathway. A view of this sort does not involve any assumptions concerning the physical or chemical nature of the mechanism by means of which transmission across the synapses is brought about.[42]

In the above discussion, reference has been made to the 'strength' of neural volleys, and the discussion has revolved about the question of the places in the pathways at which this strength might be reduced as a result

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of various tissues is reviewed by Sherrington, *op. cit.*, 45, 65-99, and by R. Dodge, The problem of inhibition, *Psychol. Rev.*, 33, 1926, 6.

[37] C. S. Sherrington, *The Integrative Action of the Nervous System*, 1906, 54-60, 64-65.

[38] *Op. cit.*, 65.

[39] *Op. cit.*, 14-18; R. S. Creed, D. Denny-Brown, J. C. Eccles, E. G. T. Liddell, and C. S. Sherrington, *Reflex Activity of the Spinal Cord*, 1932, 5.

[40] R. Granit, The physiological significance of the retinal synapses, *Report of Discussion on Vision by Physical and Optical Societies, London Physical Society*, 1932, 263-271; C. H. Graham, Vision; (III) Some Neural Correlations, *Foundations of Experimental Psychology*, 1934, 827-879.

[41] R. Dodge, *Human Variability*, 1931, 15.

[42] For discussions of recent work upon the nature of transmission at synaptic and neuromuscular junctions see papers by L. Lapicque, A. M. Monnier, A. Rosenblueth and H. H. Dale in *Cold Spring Harbor Symposia on Quantitative Biology*, IV, 1936, The Biological Laboratory, Cold Spring Harbor, 98-149.

of decreased excitability. It is necessary to consider whether the above use of the term 'strength' and the conception of a reduction in the strength of neural volleys as a result of a locally depressed state of excitability in the pathway, are compatible with physiological evidence concerning the transmission of the nerve impulse. As used above, 'strength' refers to that property of the succession of neural volleys which correlates, other things equal, with the intensity of the sensory stimulus and with the intensity of the resulting sensation. The work of Adrian and others has shown that this property of the neural messages is represented by the total number of discrete impulses per second transmitted by an axon or group of axons.[43] In terms of this doctrine the inhibitory or refractory after-effects which are expected to lead to a reduction in 'strength' of the subsequent nerve-volleys must be such as to reduce the total number of impulses transmitted per second in a response to a given stimulus. Such a reduction might be brought about by a reduction in the total number of fibers conducting or in the frequency of response of individual fibers.

It is not difficult to understand how such changes might be brought about as a result of local reductions in excitability at receptors or at synaptic junctions. At receptors, for example, a loss of excitability would be equivalent to a reduction in the effectiveness or intensity of the stimulus. As a result there would be fewer receptor-cells in the retinal area stimulated for which the stimulus would be super-liminal, and those which were stimulated would discharge at a slower rate. The net result would be a reduction in the total number of impulses discharged per second.

Similar effects can be regarded as occurring upan transmission of volleys of impulses across groups of synapses characterized by a state of reduced excitability. The situation may be conveniently formulated in terms of Sherrington's doctrine of the central excitatory and inhibitory states which may be present at a synapse.[44] If a given train of impulses arrives at a synapse, it may be expected to produce a central excitatory state (*c.e.s.*) of a given magnitude. But if the magnitude of the central inhibitory state (*c.i.s.*) is greater than it is in the resting condition, the resultant *c.e.s.* will be reduced. Since the *c.e.s.* at the synapse may be regarded as the stimulus for the series of impulses released in the following stage of conduction, there will be a corresponding reduction in the total number of

[43] E. D. Adrian, *The Mechanism of Nervous Action*, 1932, 8-60; H. K. Hartline and C. H. Graham, *Nerve impulses* from single receptors in the eye, *J. Cell & Compar. Physiol.*, 1, 1932, 277-295.

[44] R. S. Creed, D. Denny-Brown, J. C. Eccles, E. G. T. Liddell, and C. S. Sherrington, *Reflex Activity of the Spinal Cord*, 1932, 32 f., 41-46, 87-90, 101-103. See also J. F. Fulton, *Muscular Contraction and Reflex Control of Movement*, 1926, 236-363.

impulses per second initiated in the next neurone-stage. In the case of certain synapses, the *c.e.s.* may even be too weak to serve as a stimulus for the next neurone. There will be a failure of transmission across the synapse and consequently a reduction in the total number of fibers which are conducting at the next neurone-stage. The notion of the intensity of visual sensation being reduced as a result of the presence of regions of reduced excitability in the visual pathway is thus compatible with physiological evidence concerning the nature of transmission in neural pathways.

(C) The relative location of the cortical processes underlying successive stimulus-objects compared. In Köhler's original theory of the negative time-error it was assumed that the trace due to the first stimulus in a comparison pair was localized in the same region in which the brain-event due to the second stimulus appeared, and that the process of comparison depended on the transition from the condition represented by the trace to that initiated by the second stimulus.[45] The results of the present study suggest, as has already been mentioned by Lauenstein,[46] that the physiological process underlying comparison may occur when the trace due to the first stimulus and the brain-event due to the second stimulus are localized in different places. In view of the fact that the successive stimuli impinged on opposite sides of the retina, it must be supposed that the corresponding neural volleys first appeared in the brain in the projection areas of opposite hemispheres. Moreover, according to one of Köhler's hypotheses, the brain-events correlated with two phenomenological objects differently localized in space are localized in different places in the brain.[47] On this basis it would be necessary to suppose that the brain-events associated with the successive stimulus-objects, which appear on opposite sides of a fixation point, are localized in different places. Nevertheless, the process of successive comparison readily occurred and negative time-errors resulted. It seems unnecessary, therefore, to assume a common localization of the brain-trace due to the first stimulus and the brain-event due to the second stimulus in order to explain the process of successive comparison and the occurrence of negative time-errors.

An additional objection to such an assumption may be mentioned. If the brain-event due to the second stimulus appeared in exactly the same region in which the trace due to the first stimulus is located, then we should always

[45] W. Köhler, Zur Theorie des Sukzessivvergleichs und der Zeitfehler, *Psychol. Forsch.*, 4, 1923, 134-139.

[46] O. Lauenstein, Ansatz zu einer physiologischen Theorie des Vergleichs und der Zeitfehler, *Psychol. Forsch.*, 17, 1932, 136.

[47] W. Köhler, *Gestalt Psychology*, 1929, 64, 224-228.

expect an 'upward transition' in this region, that is to say, judgments of rising intensity *only*. For the effects produced by the second stimulus would be added to the cortical after-effects of the first stimulus (*i.e.* trace no. 1) even though stimulus no. 2 were much weaker than stimulus no. 1. This consequence obviously conflicts with the facts even in the experiments in which the successive stimulus-objects are localized at the same place on the screen. Although there may be a preponderance of judgments of rising intensity in a given series (negative time-error), judgments of falling intensity and of equality also occur. Another type of argument against this assumption has been given by Pikler.[48] It seems plausible to conclude that in the process of successive comparison, even when the successive stimulus-objects are phenomenologically localized in the same place, the brain-trace due to the initial stimulus of a comparison-pair becomes displaced in space from the region in the brain in which the event associated with the second stimulus-object is localized.

Limitations of the Present Study

In Köhler's investigations of the negative time-error, it was discovered that, as the number of judgments made by a given *S* increased, the character of the time-error curves changed.[49] Since we wished to avoid this 'repetition effect' in the present study, the procedure adopted by Köhler of combining the results obtained from a group of *Ss* was followed. This procedure makes it possible to obtain sufficient data for analysis without securing any considerable number of reports from a single *S*. The procedure also involves, however, certain limitations. In the first place, it makes the determination of the statistical reliability of the results more difficult. Estimates of reliability cannot be made on the basis of the variations found among individuals, but requires the repetition of the experiment with new groups of *Ss*. In the present study, for example, the nature of the time-error curves obtained when the successive stimuli impinged upon different retinal areas has been determined for two separate groups (Curves A and C). The determination of the type of curve obtained when successive stimuli impinge on identical retinal areas (Curve B) was, however, made only for one group of *Ss*. A second limitation of this practice of analyzing the combined data of a group is that it fails to acquaint us with the specific time-error phenomena which occur in the individual *S*. It is possible that the curves obtained from the combined data of a group provides a distorted picture of the effects occurring in individuals or obscures recognition of different varieties of effect that may occur in different *Ss*. It is desirable, therefore, that experiments such as reported here should also be performed under conditions that will provide sufficient

[48] J. Pikler, Theorie des Gedächtnisses, mit einer kritischer Anwendung auf der W. Köhlerischen physiologische Theorie des Sukzessivvergleichs, *Schriften zu Anpassungstheorie des Empfindungsvorganges*, 1926, 5, 27 f.

[49] W. Köhler, Zur Theorie des Sukzessivvergleichs und der Zeitfehler, *Psychol. Forsch*, 4, 1923, 153 f.

data for the determination of the time-error curves of individuals. Since the time when the present experiments were undertaken special studies upon the 'repetition effect' discovered by Köhler have been made.[80] It is possible that the results of such studies will facilitate the design of experiments in which correction may be made for this effect and thus permit the carrying out of a longer series of observations by the individual *S*.

A further point is to be noted with respect to the conditions of the present experiments. The task presented to the subject of comparing the brightness of areas appearing in the periphery of the visual field is not easy. It is therefore possible that chance factors may play a relatively large rôle in the reports and will thus serve to reduce the prominence of the effects produced by the variables explicitly controlled. This possibility indicates the desirability of using *Ss* trained in extra-foveal observation and emphasizes the need for repetition of the experiments with additional groups of *Ss* under new laboratory conditions.

SUMMARY

The purpose of the present study was to obtain evidence concerning the neurological level of the processes on which the time-errors depend. Two specific questions were raised. Do negative time-errors occur when conditions are so arranged that the underlying physiological factors cannot be localized in neural pathways leading to the brain, and are time-error components contributed to the time-error curve as a result of the successive activity of the same afferent pathway to the brain? The modality of vision seemed to be most appropriate in obtaining answers to these questions in view of the fact that the impulses from opposite sides of the retina are transmitted to the brain over separate pathways and arrive at the cortex in projection areas of opposite hemispheres.

The main results and conclusions are as follows:

(1) Evidence was obtained that negative time-errors occur even when neural volleys produced by successive stimuli reach the brain by separate routes. A discussion of the possible neural factors involved led to the conclusion that the underlying factors were localized in the cerebral cortex.

(2) The results obtained when the neural volleys produced by successive stimuli travel over the same afferent paths indicate that the pathways contribute a positive time-error component to the resultant time-error curve and that the magnitude of this component gradually decreases with increase in time-interval.

[80] J. G. Needham, The time-error as a function of continued experimentation, this JOURNAL, 46, 1934, 558-567; H. Woodrow, The effect of practice upon time-order errors in the comparison of temporal intervals, *Psychol. Rev.*, 42, 1935, 127-152; H. Woodrow and L. H. Stott, The effect of practice on positive time-order errors, *J. Exper. Psychol.*, 19, 1936, 694-705.

The possible rôle of different regions of the afferent pathways (receptors, nerve fibers, and synapses) in producing a positive time-error component was considered. The conclusion was reached that the positive time-errors produced in the afferent pathway are due to a local reduction in excitability following stimulation, the reduction occurring either at receptors or at synapses. This after-effect may be spoken of as an inhibitory after-effect, and, at the synapses, may be understood in terms of an increase in the central inhibitory state postulated by Sherrington. Existent physiological data do not permit conclusions as to whether or not the inhibitory after-effect is localized specifically at receptors or at synapses, or in both types of tissue.

(3) The bearing of the results on the question of the relative location in the brain of the processes on which successive comparison depends was discussed. It was concluded that the 'brain-trace' representing the initial stimulus-object is not spatially coincident with the brain-process representing the second stimulus-object.

STUDIES IN VIBRATORY SENSIBILITY

By LOUIS H. COHEN,[1] Worcester State Hospital, and STANLEY B. LINDLEY, University of Minnesota

The nature of the relationship between vibratory sensibility and the various sense modalities is a problem which has received little more than theoretical attention. On the basis of the comparatively few experimental and clinical investigations which have been undertaken in the past, numerous theories concerning vibratory sensibility have been advanced.

As early as 1846, the opinion was expressed by Weber that the sensation produced by very rapid successive impacts to the skin was intermediate between touch and audition.[2] Rumpf considered the sensation of vibration to arise from excitation of the cutaneous nerves which carried it to its true receptor in bone.[3] Bechterev[4] and Bonnier[5] maintained that in the nerves of all tissues there is a special kind of perceptive quality by virtue of which vibratory sensation arises. Egger believed that the cutaneous nerves played no rôle,[6] a view to which Dweitschenko subscribed.[7] Stcherbak and Naumann were of the opinion that the skin acted as an enhancer of vibrations, the reception of which was confined, however, to bone.[8]

Treitel concluded that vibratory sensation was different from touch, that it was similar, rather, to the sensation of squeezing produced by pulsating tubes.[9] Noischewsky believed it to be identical with "hair sensation" which he had described.[10] On the other hand, Forli and Barrovecchio[11] and Herzog[12] considered touch and vibration

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[1] National Research Council Fellow in Medicine, 1933-1934.

[2] E. H. Weber, *Tastsinn und Gemeingefühl*, In R. Wagner's *Handwörterbuch der Physiologie*, 3, 1846, 401-588. Reprinted in Ostwald, *Klassiker der exakten Wissenschaftern*, No. 149, 1895.

[3] — Rumpf, Über einen Fall von Syringomyelie nebst Beiträgen zur Untersuchung der Sensibilität, *Neurol. Centralb.*, 8, 1889, 185, 222, 257.

[4] Bechterev, cit. by D. Katz, Über die Natur des Vibrationsinns, *Munch. Med. Woch.*, 70, 1923, 706.

[5] P. Bonnier, Sur diverses formes de paracousie, *Compt. rend. Soc. de Biol.*, 10, 1898, 851-854.

[6] M. Egger, De la sensibilité osseuse, *J. de Physiol. et de Pathol. Gen.*, 1, 1899, 511-520.

[7] D. Dweitschenko, Materialen zur Frage der Knochensensibilität, *Neurol. Centralb.*, 19, 1900, 430-431.

[8] A. E. Stcherbak and J. Naumann, Zur Frage über die Vibrationssensibilität, *Neurol. Centralb.*, 22, 1903, 894.

[9] L. Treitel, Über das Vibrationsgefühl der Haut, *Arch. f. Psychiat.*, 29, 1896-97, 633-640.

[10] K. Noischewsky, Über das Vibrationsgefühl von Treitel und das Knochengefühl von Egger, *Neurol. Centralb.*, 22, 1903, 238-239.

[11] V. Forli, and B. Barrovecchio, Ein weiterer Beitrag zur Kenntnis des Vibrationsgefühls, *Med. Klin.*, 1, 1905, 851-853.

[12] F. Herzog, Über das Vibrationsgefühl, *Deutsche Zeit. f. Nervenheilk*, 31, 1906, 96-107.

one kind of sensibility. Von Frey,[13] on the basis of work on "single-spot stimulation" with discontinuous stimuli at various rates was led to the conclusion that the pressure end-organs could also mediate these discontinuous sensations (vibration) although not as effectively as those of pressure. Goldscheider ascribed the reception of vibration to both the pressure end-organs and to the nerves of "deep" sensibility.[14] Frank[15] and Katz[16] believed vibratory sensibility to be a specific sense-modality, the latter considering it the genetic antecedent of both touch and audition. In all these investigations of vibratory sensibility the tuning fork technique was employed, and the data consist entirely of thresholds of frequency or of "felt-duration."

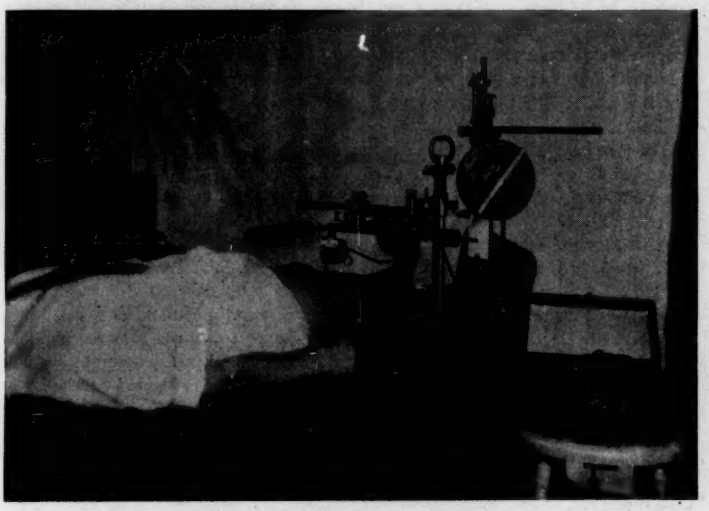

FIG. 1. PALLESTHESIOMETER IN POSITION

In the first investigation to be reported there are three features which are emphasized: (1) the utilization of a new technique for the investigation of vibration; (2) the presentation of a method whereby accurate observations of variations of vibration due to pressure and other extrinsic factors can be made; and (3) the experimental study of the effect of pressure and of pressure variations on the threshold of vibratory sensibility.

[13] M. von Frey, Physiologische Versuche über das Vibrationsgefühl. *Zeit. f. Biol.*, 65, 1914, 417-427; Das Vibrationsgefühl, eine Leistung des Drucksinns der Haut, *Sitzungsb. d. Physikal. med. Gesellsch. zu Wurzburg*, 1, 1915, 38-42; Eine Bermerkung über den sogenannten Vibrationssinn, *Zeit. f. Biol.*, 85, 1927, 539-541.

[14] A. Goldscheider, Über das Vibrationsgefühl, *Berl. Klin. Woch.*, 41, 1904, 353-356.

[15] C. Frank, Die Störungen der Vibrationsgefühle bei den traumatischen Verletzungen der peripheren Nervenstämme, *Arch. f. Psychiat. u. Nervenkrankh*, 69, 1921, 627-727.

[16] D. Katz, Über die Natur des Vibrationsinns, *Munch. Med. Woch.* 70, 1923, 706-708.

I. Effects of Pressure Variations

Apparatus. The apparatus consisted of a pallesthesiometer[17] in which there is an electromagnetically controlled rod which vibrates vertically through an amplitude regulated by a potentiometer. The frequency was 60 double vibrations per sec. The instrument operates on a 110 volt, 60 cycle, A.C. circuit. It was attached to a lever-arm which was counterbalanced by a set of weights and an adjustable chain. The pressure exerted by the instrument at its point of contact with *S* could be varied by change of the weights which were calibrated with a balance. On the lever-arm were oil-levels by which it was possible to determine and maintain a constant pressure equilibrium in a given plane. In the studies to be reported, pressure was applied vertically only. Once the instrument was placed upon a given part of the body, it could be maintained in the same position and at constant or varying pressure without being moved. "Buttons" of various sizes could be readily attached to the vibrating rod at its point of contact with the body. Fig. 1 presents a photograph of the instrument.

The threshold values are obtained in units from 0–100 on the potentiometer dial. These can be converted into units of 0.001-in. amplitude of vibration by reference to a calibration curve, but for our present purposes this is not essential, and has not been done. In this connection it must be pointed out that the calibration-curve furnished by the manufacturers must have been obtained with the vibratory rod resting against some homogeneous material or suspended in air. When resting upon surfaces of different resiliency, the absolute values in units of 0.001-in. are different, although the form of the curve remains the same as will be shown. This factor is important since an absolute threshold value in terms of the potentiometer dial reading may be the same, and the *actually delivered* amplitude of vibration may be different upon different regions of the body. In order to study the possible existence of such differences, an optical lever system incorporating a small concave mirror of 6.5-in. principle focus was placed on the vertically oscillating rod in such a way as to reflect a strong beam of light onto a moving film at a distance of 972 ± 3 mm. The conditions of photographic registration in no way modified the instrument as far as the *Ss* were concerned.

General procedure. The *Ss*, recumbent upon a mattress, were blindfolded, and were instructed to keep absolutely still. All were men between 18 and 28 yr. of age.

(A) Pressure variation. The effects of three different pressures, regulated by the weights previously described, were studied. These pressures were 50, 75, and 100 gm. Lighter pressures were not studied because, as was found in preliminary experimentation, only wavery, vague, ticklish sensations were produced for which it was impossible for *S* to establish adequate criteria for threshold determination. Pressures greater than 100 gm. were not studied because of fear that with such pressures the instrument might be injured. Three different kinds of hard rubber buttons were used; (a) the 5s (solid button, 5 mm. in diam.), (b) the 10s (solid button, 10 mm. in diam.), and (c) the 5h (hollow button with a rim 1 mm. wide, equal in area to the 5s). These dimensions were selected from those used in a more extensive study on the relationship of area and diameter of stimulating surfaces to the vibratory threshold.[18]

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[17] These instruments are manufactured and distributed by The Winchester Company, New York City.

[18] See below, p. 55 ff.

The 5s- and 10s-buttons were studied in random order on the ventral mid-thigh (13 *Ss*), on the patella (6 *Ss*), and on the medial aspect of the mid-tibia (7 *Ss*). These regions of the body were selected because of the differences of bulk of tissue overlying bone. The 5h-button was studied under the same conditions on the mid-tibia (3 *Ss*),

TABLE I

THRESHOLDS (T) AND THEIR RELATIVE RANKS (R) FOR EVERY *S* ON THE RIGHT PATELLA
(Every threshold is the average of 10 determinations)

S	5s-button						10s-button					
	50 gm.		75 gm.		100 gm.		50 gm.		75 gm.		100 gm.	
	T	R	T	R	T	R	T	R	T	R	T	R
Mo	27.7	3	24.8	2	22.4	1	56.7	3	33.5	2	23.5	1
Br	19.3	1	21.5	2	23.4	3	24.4	3	19.8	1	22.5	2
Sm	29.6	3	25.8	2	24.9	1	21.1	2.5	21.1	2.5	19.1	1
Ch	73.2	3	45.6	1.5	45.6	1.5	52.1	3	44.9	2	30.3	1
Tu	39.6	2	47.4	3	37.8	1	32.4	2	35.6	3	22.0	1
Be	32.9	3	32.7	2	23.5	1	37.3	3	32.4	2	30.0	1
Mean	37.0	2.7	33.0	2.1	29.6	1.4	37.3	2.8	32.9	2.1	24.6	1.2

TABLE II

THRESHOLDS (T) AND THEIR RELATIVE RANKS (R) FOR EVERY *S* ON THE RIGHT THIGH
(Every threshold is the average of 10 determinations)

S	5s-button						10s-button					
	50 gm.		75 gm.		100 gm.		50 gm.		75 gm.		100 gm.	
	T	R	T	R	T	R	T	R	T	R	T	R
Mo	69.5	1	74.4	3	73.3	2	65.5	1	73.8	2	76.8	3
Br	15.7	3	14.2	1	15.1	2	16.3	3	15.1	2	14.5	1
Sm	47.7	1	55.6	3	51.9	2	46.3	3	45.2	2	44.9	1
Ch	40.9	2	37.8	1	44.7	3	32.7	2	33.8	3	32.4	1
Tu	79.7	3	77.6	2	75.4	1	77.0	3	74.1	2	69.2	1
Be	51.0	3	49.2	2	46.6	1	51.8	2	52.3	3	45.8	1
Mi	33.2	1	35.8	2	37.9	3	70.8	3	50.5	2	42.8	1
Re	58.5	2	55.2	1	62.4	3	53.7	3	45.1	2	43.9	1
In	48.1	3	45.4	2	42.9	1	62.3	2	64.0	3	57.7	1
Fl	32.4	3	30.9	2	30.8	1	40.2	3	34.5	1	37.5	2
Su	45.3	1	47.5	2	48.9	3	61.4	1	66.7	3	62.0	2
Bu	39.3	2	39.0	1	43.2	3	50.4	1	52.4	2	53.5	3
Le	41.9	3	27.9	1	32.0	2	40.4	3	29.8	2	27.0	1
Mean	46.4	2.2	43.9	1.8	46.5	2	51.4	2.3	49.0	2.2	46.8	1.5

and ventral mid-thigh (3 *Ss*). Ten determinations at approximately 8-sec. intervals for each button were obtained. Between every series of 10 determinations an interval of about 2 min. was allowed to elapse.

Results. The average threshold-values obtained under each of these experimental conditions are presented in Tables I–IV. With every value is also presented the relative order of magnitude of threshold for a given button

for every *S*: the lowest average value being rated 1; the next higher, 2; and the highest, 3. These were totaled and averaged to indicate the average rank order of increasing threshold magnitude.

TABLE III

THRESHOLDS (T) AND THEIR RELATIVE RANKS (R) FOR EVERY *S* ON THE RIGHT TIBIA
(Every threshold is the average of 10 determinations)

S	5s-button						10s-button					
	50 gm.		75 gm.		100 gm.		50 gm.		75 gm.		100 gm.	
	T	R	T	R	T	R	T	R	T	R	T	R
Mi	49.3	3	38.3	2	27.4	1	40.4	3	32.8	2	28.8	1
Re	15.6	3	12.7	2	11.0	1	15.0	3	13.2	2	10.5	1
In	29.3	3	21.3	2	18.0	1	13.0	1	19.1	3	16.9	2
Fl	25.5	3	19.1	2	18.0	1	21.1	3	18.5	2	14.6	1
Su	24.2	3	21.1	2	18.7	1	21.6	3	20.8	2	17.9	1
Bu	31.9	3	25.0	2	23.9	1	24.7	3	25.1	1	26.0	2
Le	33.7	3	24.9	2	21.7	1	36.3	3	25.2	2	22.2	1
Mean	29.94	3.0	23.22	2.0	19.84	1.0	24.61	2.7	22.13	2.0	19.56	1.2

TABLE IV

THRESHOLDS (T) AND THEIR RELATIVE RANKS (R) FOR EVERY *S* ON THE RIGHT TIBIA AND RIGHT THIGH
(Every threshold is the average of 10 determinations)

	S	50 gm.		75 gm.		100 gm.	
		T	R	T	R	T	R
5h-button on tibia	*Mi*	41.5	3	33.5	2	26.7	1
	Fi	26.6	3	23.5	2	19.0	1
	Su	23.8	3	19.8	2	18.5	1
	Mean	30.7	3.0	25.6	2.0	21.42	1.0
5h-button on thigh	*Re*	49.6	3	37.9	2	35.7	1
	In	54.0	3	46.0	1	47.4	2
	Le	48.2	3	34.5	1	37.2	2
	Mean	50.65	3.0	39.52	1.3	40.12	1.7

Conclusions. From these results we draw the following conclusions.

(1) Generally, the greater the pressure, the lower is the magnitude of the vibratory threshold. These results indicate a relationship between pressure and vibration which may be psychophysiological, but which is probably due merely to mechanical factors. For, with greater pressures, there is an increased compression of the underlying soft tissues which may make them better physical conductors of vibration. That such compression of tissue also brings about a greater concentration of vibratory receptors per unit area is rather doubtful.

(2) On the thigh the differences in thresholds for the 5s-button are not appreciably different for the 50- and 100-gm. pressures. The progression of threshold-magnitude on the patella and tibia is, on the other hand, direct for the three pressures studied. The difference of thresholds

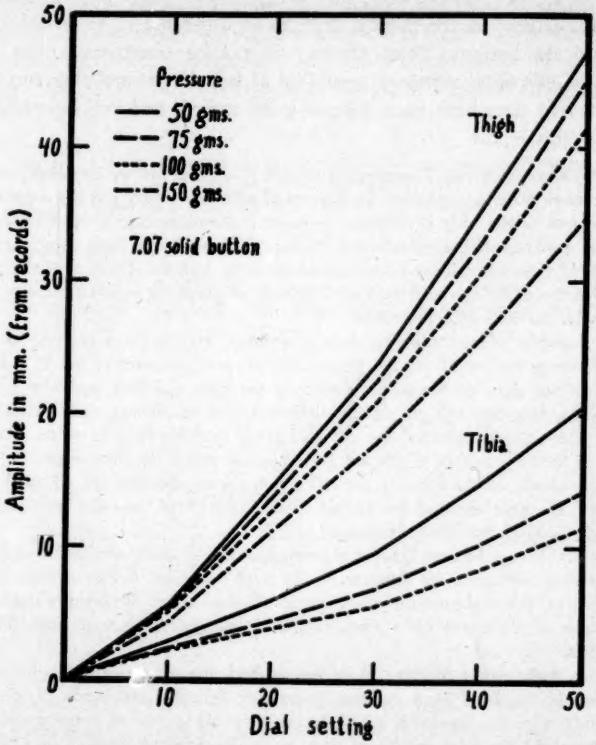

FIG. 2. AMPLITUDES OF VIBRATION ON THIGH AND TIBIA WITH DIFFERENT PRESSURES AT VARIOUS DIAL SETTINGS (PHOTOGRAPHIC REGISTRATION)

in these regions may indicate that on soft tissues compression induces greater vibratory conductivity up to a certain point, beyond which it remains constant, whereas on harder tissues this situation does not obtain.

(3) The difference of absolute value of the threshold on given body and fleshy regions may be observed for the group of *Ss*. Threshold-values are higher on the thigh than on the patella and tibia. The values on the

tibia are lower than for the patella.[19] There appears, therefore, to be an inverse relationship between vibratory threshold and thickness of overlying soft tissue in the region of threshold determination.

(4) On the thigh, the larger button appears to have a higher threshold than the smaller; on the tibia, the smaller button has a higher threshold than the larger; on the patella, there is no difference in threshold-values between the buttons. These results will not be commented upon here since the effects on vibratory sensibility of buttons (stimulating surfaces) of different sizes have been systematically sudied, and will be presented later in this report.

(B) Energy delivered. These results seemed to warrant further investigation since the question arose as to whether the amount of vibratory energy *actually delivered* to a given part of the body at different pressures could be accurately determined from the dial readings of the potentiometer alone. As has already been mentioned, the amount of vibration delivered may depend upon the anatomical nature of the region to which the vibrating rod is applied. Extensive observations were carried out, therefore, with the following procedure.

Photographs of the vibrating rod were taken with a given button, with the potentiometer dial set at 10, 20, 30, 40, and 50, and pressures of 50, 75, and 100 gm. This was done on the medial surface of the right mid-tibia, and with pressure of 50, 75, 100, and 150 gm., under similar conditions, on the right ventral mid-thigh. The 150-gm. pressure was not studied on the tibia because of the battering effect of the hard surface which was feared would injure the instrument.

As a sample of the form of the calibration curves obtained for all the buttons studied, the curve obtained for one of them (7.07s)[20] at these different pressures on both the thigh and tibia is presented in Fig. 2.

Results. It may be seen from this graph that the absolute amplitude values and the form of the curves are different on the thigh and tibia. For both these regions the effect of increased pressure is to lower the absolute values of vibration amplitudes. The form of the curve on a given region is essentially the same with different pressures.

From these results it seems clear that on both the tibia and thigh, increase of pressure is associated with decrease in actually delivered amplitude of vibration. This fact makes the previously established inverse relationship of pressure and vibratory threshold the more significant since at greater pressures smaller amplitudes of vibration than are signified by the potentiometer dial readings are associated with increased sensitivity to vibration (lowered thresholds).

Summary. The threshold for amplitude of vibration has been found in this investigation to be in an inverse relationship to variations of pressure.

[19] With the leg in extension, as was the case when these measurements upon the patella were obtained, it may be observed that the patella comes to lie deep within the condyles of the femur. In this position there is more soft tissue overlying the patella than when the knee is in a position of flexion, when it lies just below the skin.

[20] This is a round button with a plain surface, 7.07 mm. in diam. (*cf.* Table X).

This relationship seems most probably to be due to mechanical factors of increased compression of tissue which under such circumstances may become a better medium for the conduction of vibration. This is indicated by differences in vibratory threshold in regions where the soft tissue overlying bone is of different thicknesses. From a photographic study of the effects upon amplitude of vibration at varying pressures, it was found that with increase of pressure there occurs progressive decrease of amplitude of vibration delivered to both hard and soft regions of the body. These findings indicate

TABLE V

THRESHOLD DETERMINATIONS UNDER VARIOUS CONDITIONS WITH INSTRUMENT SELF-REGULATED AT THRESHOLD INTENSITY

(Average of 4 consecutive readings at 15-sec. intervals)

Subjects						
Li R. calf 5s, 75 gm.	*Li** R. calf 5s, 100 gm.	*Be* R. thigh 10s, 75 gm.	*Be* R. thigh 10s, 100 gm.	*Be* R. tibia 10s, 75 gm.	*Be* R. tibia 10s, 100 gm.	*Co* R. patella 7s±, 75 gm.
21.0	45	42.0	46.0	12.0	10.0	19.0
19.5	—	39.8	44.0	12.8	7.3	22.3
20.2	—	38.0	45.3	12.3	8.8	25.3
27.5	45	46.8	47.0	11.8	10.3	24.8
35.6	40	38.0	44.8	9.8	11.0	26.3
40.3	43	40.3	47.3	9.8	10.0	29.3
40.5	44	38.8	47.3	12.8	9.0	30.8
42.5	45	37.5	48.0	10.5	7.0	30.8
44.5	48	38.3	43.5	12.8	8.3	32.5
47.5	52	37.8	47.0	9.8	8.8	34.8
51.0	59	42.8	47.3	12.8	9.3	35.5
52.5	64	45.8	49.0	13.8	9.8	36.0
53.5	65	52.3	36.8	15.3	7.3	37.8
53.5	65	62.8	49.0	12.5	7.8	41.3
56.5	66	—	48.0	15.0	9.3	43.3

* Readings taken at 1-min. intervals.

the great importance of the investigation of such mechanical factors in experimentation on vibratory sensibility. They also indicate that the lowering of the vibratory threshold under conditions of increased pressure is of even greater significance than is indicated merely by the obtained threshold-values.

II. MODIFICATION BY CONTINUED STIMULATION

Of the numerous studies of vibratory sensibility which have appeared in the literature, none have dealt with the problem of the effects of continued stimulation. Investigations of the nature of the modification of the various other sensory functions of the individual are, on the other hand, voluminous and, beyond this, need no mention. It was found, in preliminary experimentation, that the vibratory threshold seemed to be different when pre-

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ceding stimulation had occurred. The present section is concerned, therefore, with an exploration into the nature of the relationship of continued stimulation of various intensities and duration to the vibratory threshold.

Procedure. (A) Continual stimulation at threshold intensity. S was blindfolded and the dial placed in his right hand. He was instructed to turn the dial slowly until vibration was just felt; at that point he was instructed to turn the dial back just enough for vibration to disappear, then to turn the dial until it reappeared.

The *Ss*, 3 in number, were *Li*, *Be*, and *Co*. For *Li*, the instrument with the 5s-button was placed on the right calf and observations made at 75- and 100-gm. pressure; for *Be*, the instrument with the 10s-button was placed on the thigh and on the tibia, and observations made at the same pressures; and for *Co*, the instrument with the button furnished by the manufacturers of the instrument (about 7 mm. in diam.) was placed on the patella at 75-gm. pressure, and observations made. Readings of the dial were taken at 15-sec. intervals by *E*.

TABLE VI

EFFECT OF PROLONGED STIMULATION ON THRESHOLD-VALUES

(Right Patella; pressure 50 gm.; 7-mm. button; stimulation-duration 1 or 2 min.)

Rest-period 2 min.; stimulation 2 min.						Rest-period 1 min.; stim. 1 min.		
Co			*Li*			*Li*		
T	Dial-reading	T	T	Dial-reading	T	T	Dial-reading	T
32.7	15	23.2	23.6	15	23.0	22.0	15	22.0
	20	29.2		20	22.2		20	21.8
	0*	26.6		0	19.6		0	20.6
	30	39.6		30	24.4		30	25.2
	20	35.8		20	30.8		20	25.4
	15	37.7		15	26.8		15	25.2
	0	32.9		0	25.4		0	19.8
	30	37.4		30	27.6		30	26.2
	0	30.3		0	25.4		0	22.2
	50	39.5		50	29.8		50	32.8
	0	32.9		0	22.4		0	28.8

* Stimulation at 0 indicates that the instrument was kept on the skin for the stated interval, but was not vibrating.

Results. Table V presents the results obtained. From these results it may be seen that in four of the series there is a sharp rise in threshold. In two of the three remaining series rises occur, but are quite slight. In the one remaining series the thresholds diminish slightly.

It is indicated from these results that the effect of continual stimulation upon the tibia is to increase or decrease the threshold only slightly, but that on the soft tissues, with one exception, a rise in threshold occurs. The increases are of different orders of magnitude. It appears, therefore, that generally the 'adaptation' to vibration is greater in soft tissue regions. The

patella, upon which there is not as much soft tissue as upon the thigh, but considerably more than upon the tibia, seems to occupy an intermediate position with respect to suspectibility to 'adaptation.'

(B) Prolonged stimulation at various intensities. In this experiment 5 successive threshold determinations were obtained, a rest-period of known duration was given, then there occurred a period of stimulation for 2 min. at a certain dial-reading, fol-

TABLE VII

EFFECT OF WEAK AND STRONG STIMULATION JUST BEFORE DETERMINATION OF THESHOLD
(Every threshold-determination was preceded by 1 min. of stimulation and 1 min. of rest; pressure, 75 gm., 10s-button)

S	Place	T	Dial-reading	T	Dial-reading	T	Dial-reading	T
Mo	Thigh	33.2	20	33.7	50	42.9	0	32.7
Bu	Thigh	17.3	20	19.6	50	29.3	0	19.6
Sm	Patella	22.3	15	21.3	50	31.1	0	23.4
Ch	Patella	66.6	20	53.5	50	65.3	0	60.4
Tu	Patella	41.3	20	42.9	50	51.0	0	49.1
Be	Patella	33.6	20	33.7	50	39.2	0	37.2
	Mean	35.72	20	34.12	50	43.13	0	37.07

lowed immediately by another series of 5 determinations. The observations were made on the right patella, at 50-gm. pressure, with the 7-mm. button, on 2 *Ss*. The series was interrupted by intervals in which no stimulation (indicated by O) occurred. These observations were repeated with stimulation intervals of 1-min. duration. The data are presented in Table VI.

It appears from these data that as an effect of preceding stimulation, under these conditions, the subsequently obtained thresholds are higher; generally, this effect is

TABLE VIII

EFFECT ON THESHOLD-VALUES OF 1-MIN. STIMULATION AT SUB-THRESHOLD, THRESHOLD, AND SUPRA-THRESHOLD INTENSITIES
(*Do*, right tibia)

Button	Pressure (in gm.)	T	Dial-reading	T	Dial-reading	T	Dial-reading	T	Dial-reading	T
10s	50	20.9	21	16.7	15	22.4	50	27.1	0	16.2
10s	75	16.2	16	18.4	15	18.6	50	25.3	0	16.5
10s	100	18.2	18	20.7	15	19.5	50	28.6	0	18.7
7.07h	100	14.0	14	20.8	15	18.7	50	25.3	0	17.1

greater, the more intense is the preceding stimulation. After rest-periods (no stimulation) the threshold-values invariably fall. These effects are the same when the rest-periods are of 1- or 2-min. duration.

In order to attack this problem in a more systematic manner, 6 *Ss* were studied under the same conditions. The instrument was, however, placed on the thigh, for 2 *Ss*, and on the patella for the other 4 *Ss*. Stimulation at dial-readings 20 and 50 was given for 1 min. These data are presented in Table VII.

It may be seen from these results that when preceding stimulation is at low

intensity (dial-reading 20) the vibratory thresholds obtained immediately afterwards do not change appreciably. When preceding stimulation is, however, of high intensity (dial-reading 50) there is a definite rise in the threshold. Following a period of no stimulation the threshold again falls, tending to regain its normal value.

For 1 *S*, the intensity of stimulation was set at threshold-value, and at dial-readings 15 and 20. The pressure also was varied and one other different button (7.07h) used. These data are presented in Table VIII.

TABLE IX

EFFECT ON THRESHOLD-VALUES OF 1-MIN. STIMULATION AT DIAL-SETTINGS 30, 40, AND 50
(Right Tibia, 10s-button, pressure 75 gm.)

Days		T Initial	Dial-setting	T	Dial-setting	T	Dial-setting	T	Dial-setting	T
Su	1	16.0	30	18.8	40	39.3	50	35.3	0	34.5
	2	24.1	30	38.3	40	33.6	50	33.6	0	32.4
	3	32.2	30	34.3	40	36.8	50	32.8	0	30.4
	Mean	24.1	30	30.5	40	36.6	50	33.9	0	32.4
Sm	1	14.7	30	17.2	40	22.8	50	27.9	0	16.6
	2	15.2	30	27.0	40	20.2	50	26.4	0	20.4
	3	16.2	30	20.5	40	24.0	50	21.8	0	19.6
	4	24.9	30	30.5	40	35.2	50	32.2	0	23.4
	Mean	17.8	30	23.8	40	25.6	50	27.1	0	20.0
Ye	1	19.6	30	31.7	40	38.9	50	38.2	0	37.3
	2	41.6	30	40.5	40	43.8	50	44.4	0	41.2
	3	45.2	30	39.8	40	43.0	50	45.3	0	42.9
	4	35.6	30	39.5	40	39.5	50	40.4	0	33.3
	Mean	35.5	30	37.9	40	41.3	50	42.1	0	38.7
Mean (N=11)		25.9	30	30.7	40	34.3	50	34.4	0	30.2

The same conclusions indicated in the discussion of Table VII may be observed to obtain in this *S* with the additional observation that variations of pressure do not seem to alter the validity of this generalization.

In still another experimental variation, 3 *Ss*, one of whom was tested on 3 different days, and the other 2 of whom were tested on 4 different days, were studied at dial-readings 30, 40, and 50. The instrument was placed on the tibia with the 10s-button, at 75-gm. pressure. The data obtained are presented in Table IX. It may be seen that under these conditions supra-threshold stimulation also produces a rise of threshold.

Summary and conclusions. In this investigation the effects of stimulation on the threshold of vibratory sensibility was studied. There is an indication that the effects are somewhat, though not markedly, different on different regions of the body, and that they are independent of size of the stimulating surface and of variations of pressure, and within limits, of duration of

preceding stimulation. In general, stimulation at or near threshold intensity has rather little influence on the threshold-value unless it is continual. On the other hand, stimulation at supra-threshold intensities tends to increase the threshold-value.

III. Effect of Size

The rôle played by the cutaneous sense modalities on vibratory sensitivity has been indicated only partially in our first study in which it was shown that the effect of increased pressure generally is to lower the vibratory threshold. In the present section the problem of the effect of touch, or more accurately, of touch combined with pressure, is attacked. Touch *per se*

TABLE X

Diameters and Areas of Contact Buttons

(In the hollow buttons a ring 1-mm. wide made contact with skin)

Button		Diameter (in mm.)	Area in contact with skin (in sq. mm.)
Solid base	5s	5.00	19.635
	7.07s	7.07	39.270
	8.66s	8.66	58.905
	10s	10.00	78.540
Hollow base	5h	7.24	19.635
	7.07h	12.50	39.270
	8.66h	18.74	58.905
	10h	25.00	78.540
Solid base	7.24s	7.24	41.169
	12.5s	12.50	122.729
	18.74s	18.74	275.823
	25s	25.00	490.875

could not be studied, as has already been noted, since a certain amount of pressure of the instrument upon the skin is essential for the establishment of adequate criteria for threshold determination. In the work to be reported here the pressure with which the instrument was applied to the surface of the body was 75 gm., unless otherwise stated, from which it is to be assumed that the subcutaneous as well as the cutaneous tissues were involved in stimulation.

Apparatus. The pallesthesiometer, described above, was used with solid and hollow buttons of various sizes, which were constructed of hard rubber. All of the buttons were made in such a way that they could be quickly fastened to or removed from the end of the oscillating rod. Three sets of 4 buttons were made. Of the 12 buttons, two sets of 4 were equal in area of surface of contact with the skin and two sets of 4 were equal in diameter. For example, as is shown in Table X a solid button of 5-mm. diam. was equal in area (19.635 sq. mm.) to a button of outside diameter of 7.24 mm., which had a rim 1-mm. wide around its circumference. The inside diameter

of this hollow button was 5.24 mm. For convenience this hollow button was named the '5h'-button which meant that it was equal to the 5s-button in area but not in diameter. Similarly, there was made a solid button 7.24 mm. in diameter, equal in diameter but not in area of surface to the 5h-button. Thus, the 5s- and 5h-buttons were equal in area, and the 5h- and 7.24s-buttons equal in diameter. Each of the other buttons was constructed with similar relationships, as shown in Table X.

TABLE XI

THRESHOLDS (AVERAGE OF 5 DETERMINATIONS) ON RIGHT CALF OF *Li*
(Twelve buttons, five days, pressure of 75 gm.)

Day	5s-button		7.07s-button		8.66s-button		10s-button	
	T	R	T	R	T	R	T	R
1	12.3	1	15.9	2.5	16.7	5	15.9	2.5
2	28.1	12	25.2	8.5	27.9	11	23.7	6.5
3	48.2	10	44.6	6	46.8	9	46.7	8
4	34.5	10	37.5	12	26.2	2	34.1	9
5	42.7	11	31.5	2	31.0	1	33.2	5
Av.	33.2	8.8	30.9	6.2	29.7	5.6	30.7	6.1
	5h-button		7.07h-button		8.66h-button		10h-button	
	T	R	T	R	T	R	T	R
1	17.4	6	19.4	7	21.4	9	23.0	10.5
2	25.2	8.5	23.6	5	20.6	2	22.2	3
3	49.2	12	44.5	5	38.7	2	39.2	3
4	34.7	11	—	—	27.3	5	26.9	4
5	38.3	8	32.0	3	37.4	7	32.9	4
Av.	33.0	9.1	29.9	5.0	29.1	5.0	28.8	4.8
	7.24s-button		12.5s-button		18.74s-button		25s-button	
	T	R	T	R	T	R	T	R
1	16.3	4	20.2	8	24.4	12	23.0	10.5
2	26.8	10	23.7	6.5	22.8	4	20.3	1
3	48.8	11	45.5	7	42.0	4	36.1	1
4	30.0	7	30.8	8	28.5	6	26.6	3
5	46.6	12	38.4	9	34.3	6	38.7	10
Av.	33.7	8.8	31.7	7.7	30.4	6.4	28.9	5.1

Procedure. The thigh and the leg were selected as the most suitable areas for experimentation. The two-point threshold is extremely high and stereognosis is very poor in these regions. Therefore there was only a slight possibility that the *Ss* could make tactual discriminations between the various buttons.

The *Ss* were supine upon a thin hard mattress resting on a stable horizontal surface. They were blindfolded and reported verbally their first perceptions of vibration. All thresholds were determined by the ascending method.

The buttons, attached to the end of the oscillator rod, were applied in random order to the same region on the skin. Unless otherwise stated, the pressure applied at the end of the rod was 75 gm.

All 12 buttons were used on 1 *S* on 5 different days. The region studied was the medial surface of the calf of the right leg, over the medial head of the gastrocnemius muscle or the medial edge of the soleus muscle.

Results. The data are presented in Table XI in the form of average threshold-readings (dial-readings) and each button is ranked in accordance with the size of the threshold for each day. Although not clear-cut, there was a noticeable tendency for lower thresholds with the use of the largest buttons. The 7.07h-, 8.66h-, 10h-, and the 25s-buttons produced the lowest average thresholds and the best average of daily rankings. The highest average thresholds and the poorest average of daily rankings resulted from the use of the smaller buttons, 5h, 5s, and 7.24s. The inconsistencies of the results are not explainable on the basis of any data that we have at hand.

TABLE XII

THRESHOLDS (AVERAGE OF 10 DETERMINATIONS) ON RIGHT TIBIA
(Eight buttons, pressure of 75 gm.)

S	Buttons (solid)				Buttons (hollow)			
	5s	7.07s	8.66s	10s	5h	7.07h	8.66h	10h
Sm	18.5	21.6	22.5	21.0	17.5	22.1	24.0	28.8
Be	15.5	19.3	18.5	20.3	20.0	18.9	23.9	25.6
Wa	8.9	9.9	9.7	8.0	9.1	8.2	10.2	11.3
Ye	28.5	35.4	23.4	34.4	37.0	23.5	30.0	40.0
Ca	13.7	14.6	12.5	13.7	15.9	13.2	17.3	19.1
Av.	17.1	20.2	17.3	19.5	19.9	17.2	21.1	25.0
Gen. Av.		18.5				20.8		

Eight of the 12 buttons were used on the right tibias of 5*S*s. The portion of the tibia studied was the widest part of the subcutaneous medial surface at about the middle of the shaft. In Table XII we see that with the exception of the 7.07h-button the use of the larger buttons resulted in higher average thresholds than resulted from the use of the smaller buttons. If we could ignore the 7.07s- and the 7.07h-buttons we would see that the average threshold gradually increased from 17.1 for the 5s-button to the maximum of 25.0 for the 10h-button. In every case the use of the 10h-button resulted in the highest threshold. Photographic records showed that the smaller solid buttons vibrated over the tibia with an amplitude 23 to 33% less than the amplitude of the corresponding hollow buttons, despite the lower thresholds obtained by using the smaller solid buttons. The records also showed a consistent increase in amplitude of vibration with an increase in the diameter of the solid buttons used. We have no explanation to offer for this apparent paradox.

The preceding experiment was repeated on the right thighs of the same 5Ss. The exact site of application of the buttons was the anterior surface of the thigh over the rectus femoris muscle, about two-thirds of the distance from the anterior superior spine of the ileum to the patella. When

TABLE XIII

THRESHOLDS (AVERAGE OF 10 DETERMINATIONS) ON RIGHT THIGH
(Eight buttons; pressure 75 gm.)

S	Buttons (solid)				Buttons (hollow)			
	5s	7.07s	8.66s	10s	5h	7.07h	8.66h	10h
Sm	65.9	63.0	71.1	70.1	63.7	62.3	47.6	42.9
Be	39.9	41.8	47.4	62.6	42.1	33.6	30.9	41.6
Wa	13.4	14.4	14.6	14.4	15.9	16.1	14.1	14.0
Ye	70.8	68.1	69.7	65.7	71.0	68.9	67.9	32.9
Ca	19.2	16.3	15.0	17.0	21.0	15.3	17.7	18.4
Av.	41.9	40.8	43.6	46.0	42.8	39.3	35.7	30.0
Gen. Av.		43.0				36.9		

used on the thigh the larger buttons produced lower thresholds than did the smaller buttons (Table XIII). The average thresholds of 35.7 for the 8.66h-button and 30.0 for the 10h-button were the lowest obtained. There is no consistent relationship between size and threshold in the case of the

TABLE XIV

THRESHOLDS (AVERAGE OF 10 DETERMINATIONS) ON LEFT THIGH
(Six buttons; vibrator applied by hand)

S	Buttons											
	7.07s		7.07h		12.5s		8.66s		8.66h		18.74s	
	T	R	T	R	T	R	T	R	T	R	T	R
Li	17.0	5	16.5	2	16.2	1	17.1	6	16.6	3.5	16.6	3.5
Fl	19.1	3	18.9	2	19.7	4	24.9	6	20.5	5	18.0	1
Ka	24.2	5	22.6	1	24.7	6	23.8	3.5	23.6	2	23.8	3.5
Di	20.2	1	30.9	6	26.0	3	27.8	5	23.3	2	26.8	4
Do	34.5	5	35.3	6	30.5	1	33.3	3	31.3	2	33.4	4
Ha	10.0	6	8.3	3	8.4	4	9.7	5	8.1	2	7.1	1
Sh	15.8	4	16.1	5	15.2	2	16.9	6	15.4	3	14.6	1
Av.	20.1	4.1	21.3	3.4	20.1	3	22.0	4.9	19.8	2.8	20.0	2.6

four solid buttons. We have no adequate explanation for the fact that the larger hollow buttons produced the highest thresholds when used over bone and the lowest thresholds when used over soft tissues. Photographic records showed no consistent or meaningful differences in amplitude of vibration between the buttons when they were used on the thigh.

In Table XIV we see the results obtained from 7Ss when the pallesthes-

iometer was applied manually, in the manner in which the instrument would be used clinically. In this experiment the pressure could not be accurately controlled. We estimated that the pressure was approximately 150 gm. We used 6 buttons in two series of 3 buttons each. The structural relationships of the buttons can best be appreciated by reference to Table X. The vibrator was applied to the same part of the thigh that was studied in the previous experiment. The average thresholds obtained by the application of the various buttons are approximately the same. Only by reference to the averages of the rankings of the thresholds of the buttons do we perceive any differentiation. Again it is observed that the largest buttons tended to produce the lowest thresholds when used on the thigh. The smallest buttons, 7.07s and 8.66s, tended to produce the highest thresholds.

Summary and conclusions. In order to determine the part played by the sense of touch in vibratory sensibility twelve buttons differing in area of contact or in diameter were employed. The buttons were so constructed as to form four series of three buttons each. In each series were two solid buttons, having plane surfaces of contact, and one hollow button having as a surface of contact a rim one millimeter wide. The hollow button had a diameter equal to that of one of the solid buttons and a surface of contact equal in area to that of the other solid button of the series. The thigh and leg were selected as the regions for experimentation because of the poor discrimination of size found there.

When used over soft tissues the largest buttons, in terms of diameter, were generally found to produce lower thresholds. This finding is supported by Dunlap's observation[21] that a very large area of contact between the base of a vibrating tuning fork and the skin of the fore arm was conducive to lower thresholds for vibratory stimulation. In our study the difference was clear-cut only in comparisons of the vibratory thresholds produced by the smallest and the largest buttons. The diameter of the surface in contact with the skin seemed to be at least as, if not more, important than the size of the area in contact with the skin.

In contrast with the results obtained on the thigh and calf of the leg the use of the largest buttons resulted in the highest thresholds when used over subcutaneous bone (the tibia). Photographic records of the amplitudes of vibration of the different buttons at the same dial readings on both soft tissues and bone afforded no explanation of these paradoxical results. Neither did the records account for the varying thresholds obtained from any one region of the body.

It is suggested that the determination of laws or principles operating

[21] K. Dunlap, Palmesthetic difference sensibility for rate, *Amer. J. Physiol.*, 29, 1911, 108-114.

in this aspect of vibratory sensibility awaits the use of considerably wider ranges of sizes of vibrating objects together with the control of the pressures to a degree sufficient to maintain a constant pressure per unit of vibrating surface regardless of the size of that surface.

IV. Effect of Skin Anesthetization

In two of the investigations already reported here the rôle or contribution of the pressure and touch sense modalities to vibratory sensibility has been shown to be of some significance. It has been shown that although the effect of increased pressure is to lower the threshold-value, the explanation for this relationship seemed to rest in the operation of mechanical rather than psychological factors. The rôle of the tactile receptors, as observed by differences of area and diameter of the stimulus-button was shown to be of probable significance. In this section, the importance of the cutaneous sense organs is investigated in another manner; namely, by narcotization of the skin with local anesthetics (ethyl-chloride spray and intradermic novocaine). The subcutaneous injection of 2 cc. of 1% novocaine-suprarenin by Von Frey has been found to produce frank loss of vibratory sensation set up by faradic stimulation.[22] On the other hand, Bing has observed that with local anesthesia produced by carbon dioxide snow for 30 sec., by cocaine and suprarenin infiltration, and by ethyl chloride spray "vibratory sensation was rarely appreciably reduced."[23] He was of the opinion that the anesthetic increased the density of the tissue and in this way made it a better conductor of vibration to underlying bone.

Apparatus. In this study only the 7.07s-button was used. The pressure of the pallesthesiometer against the skin was kept constant at 75 gm.

Procedure. (A) Ethyl chloride anesthetization. (1) The instrument was applied upon the right mid-calf. After the first series, four series of 5 threshold determinations each were taken at 2-min. intervals; during the first minute of the interval, before each of these series, ethyl chloride was sprayed upon the skin, under, and in the immediate vicinity of the button. Following this, 6 series of threshold-determinations were taken at 2-min. intervals. Ethyl chloride was then sprayed again for the first minute before the last 3 series.

(2) The experiment was repeated under similar conditions except that the instrument was applied to the right ventral mid-thigh. Six series of threshold determinations were obtained, then ethyl chloride was sprayed for the next 3 series during the first minute of the 2-min. rest-interval between threshold determinations; before the last series, it was again applied.

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[22] *Loc. cit.*

[23] R. Bing, Die Knochensensibilität und ihre Untersuchung durch die Stimmgabelmethode, *Med. Klin.*, 1, 1905, 332-335.

Results. The data of (1) and (2) are presented in Fig. 3 in the solid line graphs.

From these graphs it may be seen that in both these studies, the effect of ethyl chloride anesthestization is to lower the vibratory threshold. These data need some discussion, however. The effect of ethyl chloride sprayed upon the skin in this manner was to anesthetize the skin to touch, pressure and pain stimulation. After continued spraying of ethyl chloride there occurs increased density of the sprayed region; the skin becomes hard, tough, and somewhat frosted with ice. A later effect of such prolonged

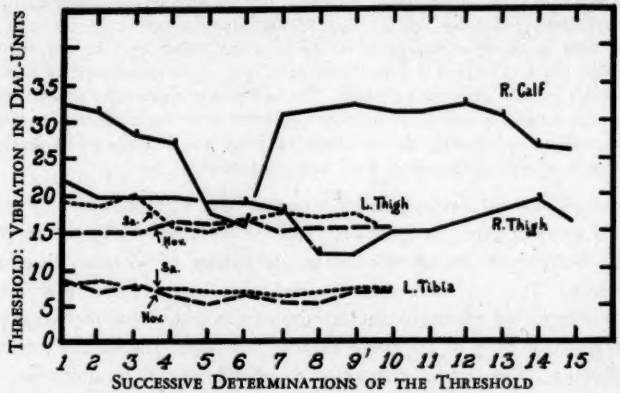

FIG. 3. EFFECT ON VIBRATORY THRESHOLDS OF ETHYL-CHLORIDE SPRAY AND NOVOCAINE WITH SALINE CONTROL.
(——— = Ethyl-chloride spray; ----- = Novocaine)

spraying of this drug was to produce fairly severe second degree burns. Both the loss of sensory receptivity and the increased density of the skin and possibly of subcutaneous tissue must be taken into account in evaluating the decrease of vibratory thresholds by this method of anesthetization. The decrease of vibratory thresholds may be ascribed either to the removal of cutaneous sensory receptors or to the operation of mechanical factors by virtue of which the density of the anesthetized area is increased. The latter possibility has been pointed out by Bing as previously noted. In either case, the superficial cutaneous nerve endings can play no sensory rôle in the reception of vibration. There is, therefore, some indication that when functioning normally the cutaneous sensory endings may actually be *unfavorable* to optimal vibratory receptivity.

(B) Novocaine anesthetization. In order to discover whether the decrease of threshold resulting from anesthetization is due to the removal of cutaneous sensibility

or to the increased density of the anesthetized region, we injected a small quantity of novocaine at the site of vibratory stimulation. We used 1% novocaine with 1/50,000 suprarenin (Parke, Davis & Co.) and injected it intradermally in quantity just sufficient to raise a bleb about the size of the 7.07s-button. As a control, physiological saline to which 1/50,000 suprarenin had been added was similarly injected. This experiment was performed on two occasions, once on the left ventral mid-thigh, and once on the left mid-tibia. Ten series of 5 threshold determinations each were taken at 2-min. intervals, after the first three of which novocaine or saline was injected.

Results. The results obtained are presented in Fig. 3 as broken line graphs. It may be seen from these graphs that the vibratory threshold-values are scarcely affected by the injection of novocaine or saline under these conditions. The slight rise or fall immediately following the injection, if significant, is transitory and may be due to local irritation at the point of the injection rather than to any specific mechanical effect. This was minimal because of the rapid assimilation of the injected fluids into the neighboring tissues. The lack of any noteworthy general effect on vibratory sensibility appears to us to be significant since the anesthesia for touch, pressure, and pain following the novocaine injection was immediate and persisted for a long time after the termination of these experiments.

Summary and conclusions. Anesthetization of a cutaneous area by ethyl chloride spray lowers the threshold-value of vibratory sensibility in that region. With novocaine anesthetization, no change of vibratory threshold is produced. The low thresholds obtained with ethyl chloride may result from the increased vibratory conductivity of the tissues due to freezing—the increased density of the tissue making it a better conducting medium for vibration. The lack of change of threshold with novocainization, on the other hand, may be more significant psychologically, since it seems demonstrated by this method that the mere removal of cutaneous sensitivity does not modify vibratory sensibility.

General Summary

The foregoing investigations have been concerned with an investigation of various aspects of vibratory sensibility. The instrument employed was an electrically controlled pallesthesiometer which permits variation of amplitude of vibration at a constant frequency of 60 per sec. It was found that the effects of increased pressure was to lower the vibratory threshold in a direct maner on various parts of the body, despite *actual diminution* of amplitude of delivered stimulation due to increases of pressure, as indicated by photographic registration. Continual stimulation at various intensities for given periods of time was found to modify vibratory threshold, independently of size of the stimulating surface, of variations of pressure, and, within limits, of duration of preceding stimulation; stimulation

at or near threshold intensity has relatively little influence on threshold unless it is continued; the effect of supra-liminal stimulation is consistently to raise the threshold. The investigation of the effects of variations in diameter and area of the stimulating surface revealed that, in general, thresholds were lower with surfaces of large diameter over soft regions, whereas the reverse is true over bony regions. Skin anesthetization with ethyl chloride spray and intra-dermal novocaine induces a lowered vibratory threshold with the former but no change of threshold with the latter. The data are evaluated in terms of the relative significance of the mechanical and psychological changes which affect sensitivity to vibration.

TIME AS A FACTOR IN THE RECOGNITION OF VISUALLY PERCEIVED FIGURES

By MORTON A. SEIDENFELD, University of Pennsylvania

The changes which may take place in the memory of visually perceived forms during the passage of time have been of interest to psychologists for many years. Interest in this problem has received increased impetus with the publication of Wulf's study in 1922.[1] This interesting study and those of Gibson, Allport, Carmichael and his colleagues, and numerous other investigators are referred to in Warner Brown's recent study which serves as a convenient summary.[2]

These earlier studies, as has been pointed out by Koffka, examined only a single function of the mechanism concerned with the memory of visually perceived forms; namely, reproduction.[3] Koffka emphasized the desirability of studying the problem using a method of recognition adapted so that long time intervals could be utilized and at the same time furnishing the individual observer an opportunity to select the stimulus-form from among other similar but non-identical forms. Although these criticisms were indicated and plans for a later investigation of the problem were mentioned by Koffka no evidence of this work has appeared in the literature to the writer's knowledge.

EXPERIMENTAL CONDITIONS

Subjects. The *Ss*, 139 in number, were all students at the college level. They consisted of the following: 69 sophomores, 50 freshmen, and 20 student nurses from the Philadelphia College of Osteopathy and Training School for Nurses. In this total group there were 106 men and 33 women. While there is a preponderance of men, a preliminary investigation revealed that there were no significant sex differences obtained, hence this factor was disregarded in the present study.

In a control study made of the ability of individuals to select identical forms by direct comparison of the stimulus-forms with the recognition-forms, there were 50 University of Pennsylvania students, 21 of whom were men, and 29 women. All these students were taking courses in psychology. In age and training both groups were essentially comparable.

* Accepted for publication May 6, 1937. The writer acknowledges his indebtedness to Professor S. W. Fernberger and Dr. F. W. Irwin for their assistance in this study.

[1] F. Wulf, Über die Veränderung von Vorstellungen (Gedächtnis und Gestalt), *Psychol. Forsch.*, 1, 1922, 333-389.

[2] Warner Brown, Growth of memory images, this JOURNAL, 47, 1935, 90-102.

[3] K. Koffka, *Principles of Gestalt Psychology*, 1935, 494-495.

All of the *Ss* participated voluntarily in the experiments and all of them were totally unfamiliar with the purposes of the study.

Apparatus. It was necessary to develop an instrument which would have the qualities of being similar to the reproduction method in that long time-intervals could be used between trials, and at the same time similar to the successive comparison method in that the *Ss* could be presented with a number of more or less similar patterns including the original stimulus-figures for selection.

In order to meet these criteria it was necessary to limit the number of forms used,

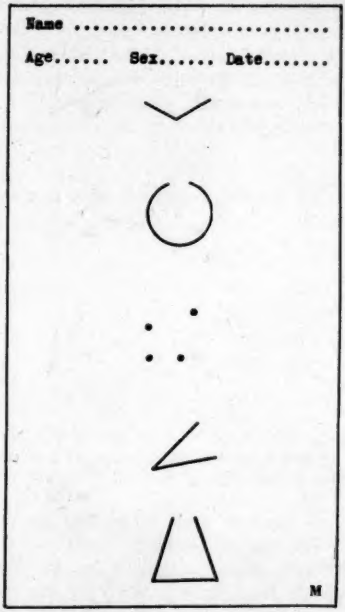

FIG. 1. THE STIMULUS-FORMS

in order that these forms might be printed and made available to every *S*. The simple figures that were adopted for this experiment as the basic forms were selected upon the basis of the experience of Irwin and the writer in an earlier study.[4] We learned that of the 6 stimulus-figures used (obtuse angle, acute angle, triangle with gap, circle with gap, dot 'square' figure, and a buttonhook figure), 5 of them were reported by practically all the *Ss* in the same manner; namely, as simple geometric forms named as indicated parenthetically above. Only the "buttonhook figure" received a wide diversity of descriptive names—this being due to the great variation

[4] F. W. Irwin and M. A. Seidenfeld, The application of the method of comparison to the problem of memory-changes, *J. Exper. Psychol.*, 21, 1937, 363-381.

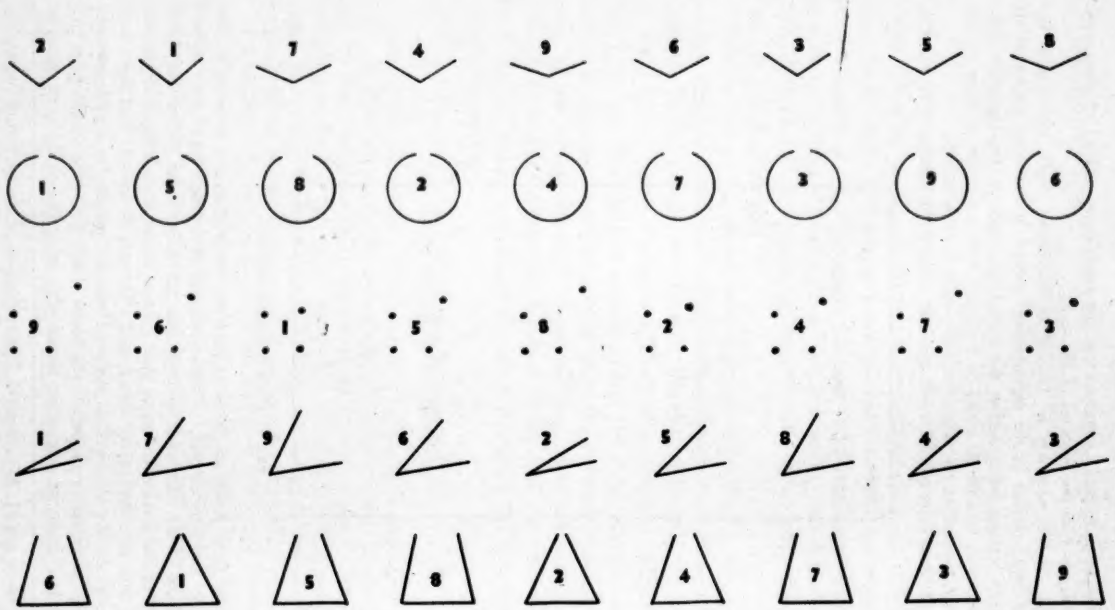

FIG. 2. RECOGNITION-FORM "AX"

in the way which the *Ss* perceived it, due probably to the fact that it was frequently associated with a number of different 'real' objects. Therefore, in the present study, the writer selected the 5 simple forms of the obtuse angle, the acute angle, triangle with gap, circle with gap, and the dot 'square' figure since the element of ambiguity was minimal.

With the selection of the 5 basic forms it was decided that a graded series of 9 figures, including the stimulus-figure, for each of the geometric forms would constitute a satisfactory means of determining any change which might take place in the ability of the *Ss* to recognize the stimulus-forms at various time-intervals.

A master-form, upon which were plotted with accurate mechanical instruments 9 variable forms of each of the 5 simple geometric figures. These figures were drawn about two and a half times the size which was finally to be used in order to insure accuracy. Each of the 9 figures belonging to every one of the 5 'type' forms was drawn identical in every aspect except the particular characteristic which was chosen to be

TABLE I

THE CHANGE IN VARIABLE CHARACTERISTIC OF RECOGNITION FORMS

Identification	Obtuse angle (deg.)	Circle w. gap (mm.)	Dot figure (mm.*)	Acute angle (deg.)	Triangle w. gap (mm.)
1	100	1	11.5	15	0.50
2	105	2	12.5	20	1.75
3	110	3	13.5	25	3.00
4	115	4	14.5	30	4.25
5	120	5	15.5	35	5.50
6	125	6	16.5	40	6.75
7	130	7	17.5	45	8.00
8	135	8	18.5	50	9.25
9	140	9	19.5	55	10.50

* Distance between 'fixed' dot in lower left corner and 'moving' dot in upper right corner measured along a 'fixed' straight line passing through the center of both.

the 'variable.' Thus every circle in the 'circle with gap' group was drawn the same size, differing only in the size of the gap. The gap was drawn so that when the figures were reduced to the desired size for printing, it would increase in succeeding figures at the rate of 1 mm. per circle. A similar scale was developed for each of the other figures with the result that when these 45 figures were photographically reduced, their variable features were distributed along a scale as indicated in Table I.

Following the drawing and the photographic reduction of the master-form, a stimulus-sheet was prepared by taking the figure which occupied the mid-position in the scale of variables (Table I, figure number 5 in all groups) and pasting it on a sheet of paper 9 x 16 cm. The mid-figure for each type of geometric form was thus placed on this sheet in a vertical column, so that a margin of about 3½ cm. on each side, and 1½ cm. between figures, was formed.

Similarly, the photographically reduced master-forms were used in preparing the recognition-sheets which consisted merely of a random distribution of each of the 19 forms in a horizontal row, made by cutting the figures out of the master-form, mixing them up, and pasting them in random order in a row with about 1½ cm. between the figures, this space was found to be large enough to eliminate any disturbance on the part of the *Ss* viewing the figures due to proximity of figures to each other.

The forms, prepared as indicated, were then printed by a photographic process which produced clear reproductions of the stimulus-form and 3 different forms of the recognition-sheet.[8] An examination of Fig. 1 will indicate to the reader the characteristics (reduced) of the stimulus-form. In a similar manner Fig. 2 will indicate the essential nature of the recognition-sheet. The numbers in each of the 45 figures have been placed there to aid the reader in comparing the form with the value for the variable characteristics given in Table I. It should be born in mind that beside this recognition-sheet, two other forms, prepared in the same manner but with different random distributions were used at different time-intervals in the experiment.

Directions. The *Ss* used were obtained in groups varying from the largest which contained 35 Ss to the smallest which had 20. Upon meeting these groups for the first time, the stimulus-sheet (Fig. 1) was passed out so that every *S* had an individual copy before him. *E* then gave the following directions.

I have just passed to each of you a small slip of paper upon which you will find 5 figures. Please examine these figures carefully so that you will be able to recognize them should you be required to do so at a later date. You will have about a minute for the examination therefore you should distribute your attention about equally over each of the 5 figures.

Following these directions the *Ss* studied the figures for a minute, then *E* said:

Please fill in the information requested at the top of this sheet. Please give all the information. Now, pass the sheet to me.

A 3-min. period was allowed to elapse from the time of gathering the stimulus-sheet until the first recognition-sheet (Form AX, Fig. 2) was passed out face downward. When these had been received by every *S*, *E* said:

I have passed to each of you a sheet which is now face downward. When you turn it over you will see 5 rows of figures. In each row there is one figure that is identical with one of those you have studied. Select that figure in each row which you consider to be identical and draw a circle around it. If you are in doubt then guess as accurately as you can and encircle the figure in each row which you believe to be the correct one. In no case should you select more than one figure in each row—nor should you fail to select one figure in each row.

Sufficient time was then allowed to insure all *Ss* opportunity to complete the task, then *E* added:

Be sure that each figure selected is carefully marked. Place your name and the date on the top of the sheet. Please pass in your sheet.

Ten minutes after the first trial, a second recognition-sheet (Form BY) was passed out in a similar manner to the above and *E* said:

I have again passed you a sheet containing a series of forms arranged in 5 rows. In each row you will again find one figure identical with those you studied originally. Please encircle the one figure in each row which you consider to be the identical one. Be sure you select one figure in each row, no more and no less.

Again sufficient time was allowed for this, then instruction was given for placing the name on the sheet and an identifying mark that would serve to indicate this to be the second trial.

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[8] Our thanks are due the H. Dando-Schaff Co., Philadelphia, Penna., for making these prints.

Seven days later a third recognition-sheet (Form CZ) was passed to the *Ss* in the group with the same instructions indicated in the second trial. Again 28 days after the original exposure, a fourth recognition-sheet was passed out to those participating. In this fourth trial all three forms of the recognition-sheet were used, the identification letters being obliterated, these forms were distributed in a random manner but with about an equal proportion of each form being represented in every group of *Ss*. In this manner the need for a new and different form for the fourth trial was eliminated.

In the control-experiment, in which the ability of the individual to select identical figures when direct comparison between the stimulus-forms and recognition-forms was permitted, the method consisted in passing to every *S* a copy of both the stimulus-sheet and recognition-sheet (Form AX) face downward, followed by *E* saying:

You have each been given a small sheet and a larger one. Please keep them face downward until directed to turn them over. Please place your name on the back of each sheet. Now turn them over and place them so that the top of each sheet is in this direction (*E* demonstrates orientation of the two sheets). Now, examine the smaller sheet containing only five figures, study these until you are quite certain you can recognize them—when you are satisfied you can do so, turn your attention to the larger sheet, where you will find 5 rows of figures, similar to the ones on the smaller paper. One figure in each of the 5 rows is identical with one of those on the smaller sheet. Select the figure in each row which you consider to be the identical one—remember, you are free to look at the small sheet anytime you wish for purpose of comparison, but do not measure, overlay the sheets, or make any marks except for the purpose of encircling the figures on the large sheet that you consider to be the identical ones with those on the smaller one.

Such additional information as needed was then obtained and the sheets collected.

Results

The results of this study are of necessity extensive because of the number of forms used and the several time intervals involved. The essential statistical results have been tabulated. In Table II the reader will find the significant statistical values obtained for every one of the 5 forms studied. Later tables deal with necessary comparative data and will be discussed as we approach the specific relationships with which the table is related.

In examining Table II it should be kept in mind that the stimulus-figures consisted of (1) an obtuse angle of 120° which in the recognition-sheet was placed in a random order distribution of angles ranging from 100° to 140° in intervals of 5°; (2) the 'gap' in the circle with gap was 5 mm. in the stimulus-figure, with a range of from 1 mm. to 9 mm. by 1-mm. intervals in the recognition-form; (3) the distance between 'fixed' dot and 'variable' dot was 15.5 mm. in the stimulus-figure, with a range of from 11.5 to 19.5 mm. in 1-mm. gradations in the recognition-form: (4) the acute angle (stimulus) was 35° and the range was from 15° to 55° by 5° steps; and (5) the 'gap' in the 'triangle with gap' was 5.5 mm. with a range of from 0.5 to 10.5 mm. in steps of 1.25 mm.

We were primarily interested in the changes that were taking place in the variable elements of the several figures during the course of time

studied. Since this change was evidenced most clearly in the obtained averages, it was essential to establish the reliability of the differences. Having obtained the sigma of a difference between any two means, it was possible

TABLE II

STATISTICAL VALUES OBTAINED FOR EACH FORM

Statistical measure	Trial	Obtuse angle (deg.)	Circle gap (mm.)	Dot fig. (mm.)	Acute angle (deg.)	Triangle gap (mm.)
N	3 min.	139	139	139	138	139
	10 min.	139	139	138	139	139
	7 days	130	130	130	130	130
	28 days	130	130	130	130	130
Mean	3 min.	124.24	5.14	15.84	35.29	5.61
	10 min.	123.92	5.06	15.91	34.78	5.69
	7 days	124.35	4.52	15.57	32.54	5.63
	28 days	122.62	4.44	15.48	33.77	5.34
S.D.	3 min.	.82	.16	.12	.59	.12
	10 min.	.91	.14	.13	.52	.13
	7 days	.90	.15	.15	.62	.15
	28 days	.96	.16	.16	.63	.16
V	3 min.	7.75	37.64	8.88	19.50	25.61
	10 min.	8.63	32.82	9.41	17.66	26.08
	7 days	8.30	38.73	10.75	21.75	29.43
	28 days	8.94	40.77	11.50	21.26	34.11

to compute the critical ratio, which would serve as an index of the probability of the differences being significant.[6] These values of the critical ratios of the various trials for each form, and the probability of the differences being significant are tabulated in Table II.

TABLE III

CRITICAL RATIONS OF THE DIFFERENCES OF MEANS AND THE PROBABILITY OF SIGNIFICANT DIFFERENCES

	Obtuse angle	Circle w. gap	Dot figure	Acute angle	Triangle w. gap
$C.R._{1-2}$	0.41	0.67	0.58	1.09	0.73
Probability	0.66	0.75	0.72	0.86	0.77
$C.R._{2-3}$	0.57	4.15	2.62	4.97	0.50
Probability	0.72	0.99	0.99	0.99	0.69
$C.R._{3-4}$	1.97	0.57	0.64	2.12	2.45
Probability	0.98	0.72	0.74	0.98	0.99
$C.R._{1-4}$	1.88	4.37	2.40	2.45	2.07
Probability	0.97	0.99	0.99	0.99	0.98

It was also desirable to obtain some measure of the consistency with which the *Ss* tended to select the forms in the several trials. The coefficient

[6] Since there was a correlation between the various trials for any given form, it was necessary to use a specialized formula for computing the sigma of a difference, which takes into account the correlation. The formula used was that discussed by J. P. Guilford, *Psychometric Methods*, 1936, 59-60.

FIG. 3. DISTRIBUTION OF JUDGMENTS OF CONTROL AND EXPERIMENTAL GROUPS

of correlation for the various trials and figures was computed in order that we might have evidence that would indicate the degree to which the *Ss* tended to select the same or a closely related form as time passed. It is sug-

gested that perhaps this is a measure of reliability but since this measure cannot really be classed as a 'test' the use of this terminology would be statistically confusing. All that is indicated at this time is the degree of intercorrelation and the consistency with which individuals selected figures

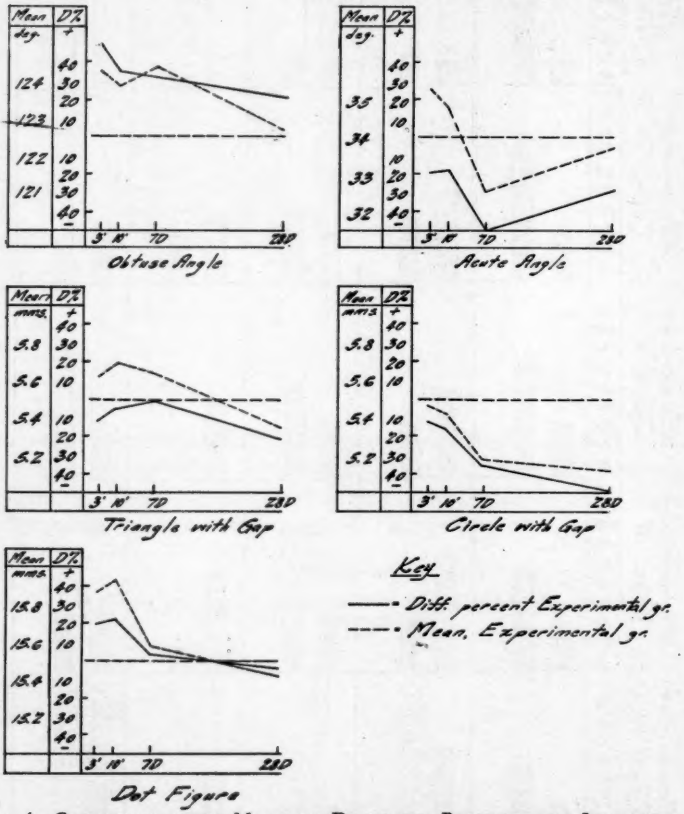

FIG. 4. COMPARISON OF THE MEAN AND DIFFERENCE PERCENTAGE OF JUDGMENTS

at the various trials. In the obtuse angle r ranged from 0.54 $\pm$0.04 to 0.66 $\pm$0.03; in circle with gap the range was from 0.50 $\pm$0.04 to 0.66 $\pm$0.03; in dot figure from 0.47 $\pm$0.05 to 0.62 $\pm$0.04; in the acute angle from 0.48 $\pm$0.05 to 0.70 $\pm$0.03, and in the triangle with gap from 0.60 $\pm$0.04 to 0.76 $\pm$0.03. Thus in all instances it is interesting to note a high degree of inter-relationship existent.

In the earlier study of Irwin and the writer we were impressed by the 'time-error-like' asymmetries of judgment that were demonstrable in the judgments made by the method of successive comparison.[7] It was thought desirable to study the results obtained by the recognition-method and observe if similar tendencies toward asymmetry of judgment were apparent.

TABLE IV

PERCENTAGE DIFFERENCE OF JUDGMENTS IN THE SELECTION OF FORMS FROM RECOGNITION SERIES

Trial	Obtuse angle	Circle w. gap	Dot figure	Acute angle	Triangle w. gap
3 min.	+49.18	−11.50	+19.54	−20.00	−11.11
10 min.	+35.59	−16.83	+21.15	−19.56	− 6.33
7 day	+32.67	−36.28	+ 1.17	−51.11	− 1.15
28 day	+21.42	−50.45	− 1.14	−27.47	−22.10

To do this the same method was used as described in the earlier study; namely, the differences were obtained between the number of judgments in which the variable factor, in each form, was chosen as greater and as less than the standard stimulus-value. These differences were then divided by the total number of greater and less judgments. These values, upon being multiplied by 100, yield 'percentage differences' similar to those used in many psychophysical studies. In Table IV will be found these 'percentage differences' for the various forms and for the different time-intervals. An inspection of this tabular list of the results and of Fig. 4, which shows the results in a graphic form, indicates that by the method of recognition a 'time-error-like' phenomenon occurs, very similar in nature to that previously reported by Irwin and the writer as occurring in the judgments made by the method of successive comparison.[8]

Since we were interested, not only in demonstrating the presence or absence of change in the ability to recognize visually perceived forms with the passing of time, but also in obtaining some idea of the rate and direction of such changes, if they existed, it was necessary to obtain some measure that would show this. Inasmuch as the mean value computed for each figure and for each trial deviated in one direction or the other from the standard value of variable characteristic in each figure studied, it was a simple matter to compute the difference between the standard and the mean judgment of any given trial, this difference divided by the standard value of the variable and multiplied by 100 yielded a 'percentage deviation' from the standard which serves to indicate about how much change took place in the form recognized by the *S*s as the standard, and by the addition

[7] Irwin and Seidenfeld, *op. cit.*, 371.
[8] Irwin and Seidenfeld, *op. cit.*, 381.

of a positive or negative sign the direction of the change could be indicated. Furthermore, it was possible to compute an average rate of change for the judgments of each figure during the total time-period involved in the present study, and also an average of the rate of change found for all the figures in any given trial. Table V shows these changes.

TABLE V

THE PERCENTAGE DEVIATION OF RECOGNIZED FORMS FROM THE STIMULUS-FORM AT THE VARIOUS TIME-INTERVALS STUDIED

Trial	Obtuse angle	Circle w. gap	Dot figure	Acute angle	Triangle w. gap	Av. % change per trial
3 min.	+3.53	+ 2.80	+2.19	+0.83	+2.00	2.27
10 min.	+3.27	+ 1.14	+2.64	−0.63	+3.45	2.23
7 day	+3.62	− 9.60	+0.45	−7.03	+2.36	4.61
28 day	+2.18	−11.20	−0.13	−3.51	−2.91	3.99
Av. % change per fig. per trial	3.15	6.19	1.35	4.00	2.68	

The results obtained from the control study in which the *Ss* were required to select the stimulus-forms found on the recognition-sheet when they had available the stimulus-sheet for direct comparison, will be found in a composite tabular form in Table VI where the same statistical measures as obtained for the experimental group are reported.

In order to secure a measure of the significance of the difference between

TABLE VI

STATISTICAL VALUES OF THE CONTROL GROUP
(Direct comparison of stimulus-forms with recognition-forms)

Statistical measure	Obtuse angle	Circle w. gap	Dot figure	Acute angle	Triangle w. gap
N	50	50	50	50	50
Mean*	121.80	5.14	15.58	34.90	5.48
S.D.	3.84	0.77	0.59	1.87	0.18
V.	3.15	15.07	3.81	5.35	3.19

* These values are in the same units as used in Tables II to V inclusive.

the means of the judgments obtained by the experimental group in their first trial (3 min.) and the control group, who made direct comparisons between the stimulus- and recognition-figures, it was necessary to compute the *critical ratios* of the means of these two groups.[9] The probability of the difference being significant is also reported in Table VII.

[9] Since these values were uncorrelated, the usual form of the formula for computing the sigma of the differences was used, rather than the specialized form used with the means of the experimental group alone.

A comparison of the amount and direction of change in the recognition of the various forms between those *Ss* required to recognize the forms in the absence of the stimulus-figures and those who were able to directly compare them is desirable. As has already been indicated it is possible to get an index of the relative amount of change by computing the percentage deviation of the selected forms from the standard stimulus-forms.

TABLE VII

CRITICAL RATIOS OF DIFFERENCES OF MEANS OF THE CONTROL AND EXPERIMENTAL GROUPS

	Obtuse angle	Circle w. gap	Dot figure	Acute angle	Triangle w. gap
C.R.	2.49	0	1.85	0.61	1.08
Probability	0.99	0	0.97	0.73	0.86

The same method was again used and the values obtained for the control and experimental group. The difference between these values was computed and the ratio of the difference relative to the value obtained by the direct comparison method computed. This ratio affords us an index of the degree with which the changes in the first judgment by the recognition-method exceeded or was less than the change obtained by direct comparison. These are indicated in Table VIII.

TABLE VIII

COMPARISON OF PERCENTAGE DEVIATION OF RECOGNIZED FORM FROM STIMULUS-FORM BY METHODS OF DIRECT COMPARISON AND RECOGNITION

	Method			Diff.
Figure	Recognition	Direct comp.	Diff.	Dir. comp.
Obtuse angle	+3.53	+1.36	2.17	1.60
Circle w. gap	+2.80	+2.80	0	0
Dot figure	+2.19	+0.51	1.68	3.30
Acute angle	+0.83	−0.28	1.11	4.00
Tri. w. gap	+2.00	−0.36	2.36	6.60

In addition to the various tables that sum up the results obtained, several graphic figures have been included to indicate more clearly some of the essential facts. Fig. 3 indicates by histograms the distribution, in percentage, of the judgments of the *Ss* in the selection of the various recognition-forms during the different time-intervals studied. The judgments are grouped into three categories of "less than the stimulus-characteristic," "equal to the stimulus-characteristic," and "greater than the stimulus-characteristic." The choices of the control group are included for comparison.

In Fig. 4, the reader will find both the means and the difference percentage of judgments plotted for comparison in order that any similarities

or differences in the judgments and the change in the mean value of the variable characteristics are made readily apparent.

Discussion

Earlier experimenters using the method of reproduction in studying the changes that occur with the passing of time in the memorial image of visually perceived forms,[10] discovered that change occurred during the entire time of the experimental periods. They not only pointed out the tendency toward continuous change but they also emphasized the tendency to constancy in the direction of change.

While a similar tendency toward continuous change was found in the earlier study of Irwin and the writer where the method of successive comparison was used, as well as in the present study, the tendency toward constancy in the direction of change was generally absent.[11] Actually, our studies have indicated tendencies toward reversals of judgments at various time-intervals within the limits of the total time-periods studied. These reversals are not constant for all of the forms investigated but are characteristic of some of them. It is interesting to note that Fehrer in her investigation of the learning of visually perceived forms used Wulf's figures along with many others in a *learning situation* found no such tendency to constancy in the direction of change as indicated by the earlier writers.[12]

Similar lack of continuity in the *direction of change* seems to have been found by Hanawalt[13] in his recent report of an experiment on the recognition of visually perceived forms, in which he used several of the forms used by Wulf in his early study. When the *Ss* were required to recognize the forms rather than reproduce them, they did not show the tendency to continue the changes in the direction given in the previous trials. Zangwill, in his interesting but admittedly inadequate study of changes in the reproduction and recognition of visually perceived forms, also fails to find constancy in the direction of change.[14]

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[10] Wulf, *op. cit.;* J. J. Gibson, The reproduction of visually perceived forms, *J. Exper. Psychol.*, 12, 1929, 1-39; Gordon Allport, Changes and decay in the visual memory image, *Brit. J. Psychol.*, 21, 1930, 138-148; F. T. Perkins, Symmetry in visual recall, this Journal, 44, 1932, 473-490; and L. Carmichael, H. P. Hogan, and A. A. Walter, An experimental study of the effect of language on the reproduction of visually perceived form, *J. Exper. Psychol.*, 15, 1932, 73-86.

[11] Irwin and Seidenfeld, *op. cit.*, 380.

[12] E. V. Fehrer, An investigation of the learning of visually perceived forms, this Journal, 47, 1935, 187-221.

[13] N. J. Hanawalt, Direction in recognition. Reported at the Meeting of the Eastern Branch, American Psychological Association, April 3, 1937, Vassar College, Poughkeepsie, N.Y.

[14] O. L. Zangwill, An investigation of the relationship between the processes of reproducing and recognizing simple figures, with special reference to Koffka's trace theory, *Brit. J. Psychol.*, 27, 1937, 250-276.

It is apparent, therefore, that the method of reproduction seems to be conducive to a seeming constancy in the direction of change, while the methods of successive comparison and recognition (and even the recent results with reproduction of these forms as indicated by Zangwill), as well as the situation involved in the learning of visual forms show no such constancy in the direction of change within the various time-intervals studied.

All of the students of the problem of change in the memory of visually perceived forms have distinctly emphasized the dynamic nature of the changes which occurred. The present study, also, serves to support that thesis. If the memorial process involved underwent a general disintegrative process, random in character, then we would expect the judgments of recognized figures to become little better than guesses with the passing of time. Our results show no such tendency to be present; in general, the judgments show a distinct and reliable trend in a specific direction at any given time-period, even though reversals may occur between one interval and its subsequent trial. It is true, however, that in the most ambiguous figure studied (dot figure) the increase in the time-interval tended to produce more and more of a chance distribution—but this can be explained in terms of the difficulty with which this type of form can be made to conform to a "best Gestalt" or to an associated form (dynamic associationist viewpoint).

Thus the present study has been able to demonstrate that the recognition of visually perceived forms undergoes dynamic change with the passing of time similar to that indicated in the reproduction of visually perceived forms. It differs, however, in the lack of tendency toward constancy in the direction of change. This difference can best be accounted for by a hypothesis which may be called a "reversible gradient hypothesis."

Koffka and others have called considerable attention to the idea that there may exist in the memorial apparatus a potential gradient between the effect of a given stimulus and its successor, with which it is compared.[15] The evidence of Wulf, Gibson, and Perkins seems to lend credence if not verification of this concept.[16] Basically, however, the concept as offered by Koffka and the others presumes a consistency in the direction of change over extended time periods. Since the evidence obtained in the earlier study of Irwin and Seidenfeld,[17] the learning study of Fehrer,[18] and

[15] Kurt Koffka, *Principles of Gestalt Psychology*, 1935, 467-480.
[16] *Op. cit.*
[17] *Op. cit.*
[18] *Op cit.*

the present investigation have shown the presence of reversals of the direction of change, obviously the "gradient hypothesis" without modification is not applicable. Since there exists in the field of physiology, from whence the concept of "gradient" has received considerable support, a further concept which is known as "reversible gradient" would it not be practical to extend this viewpoint into the field of memory change?

One of the best examples of "reversible gradient" found in the field of physiology is reported by Alvarez who impressed with the observations that muscular segments taken from progressively lower regions of the gastro-intestinal tract show decreasing gradients with regard to activity, rhythmicity, the frequency of rhythmic contractions, and metabolic rate, suggested that the unidirectional spread is conditioned by higher states of excitability in progressively higher regions of the tract.[19] If for any reason, the gradients are reversed, reverse peristalsis occurs. Thus the presence of an irritant or a pathological state that serves to constantly stimulate the immediate area such as occurs in ulceration may cause a reversal of the gradient in the intestinal tract.

Countless other examples of reversals of gradient are to be found in the field of chemistry, where many chemical reactions which left to themselves may normally progress in one direction, in the presence of changes in certain physical forces, may undergo complete reversal.

Let us now consider how this may be applicable to the problem which we are considering. Evidence has been furnished to indicate that the memorial process involved in the reproduction of visually perceived forms tend to go on in a unidirectional manner within the limits of the time periods studied, while on the other hand reversals in direction occurred frequently in the memorial apparatus as evidenced by the ability of individuals to select identical figures either by successive comparison or by the method of recognition from large groups of similar figures. It is suggested, therefore, that in the method of reproduction the change is consistently in a single direction, simply because the individual in reproducing the forms is able to make these changes at a comparatively slower rate over any given time-period. Furthermore, since the changes in the figures reproduced involve the whole figure and not just a single variable of the figure, it is possible that the *Ss* have allowed the tendencies toward change in size of figures to go unconsidered as a definite factor in influencing the other changes. It is obvious, for instance, that if the *Ss*, in drawing a circle with a gap, tend to reduce the total size of the circle with each reproduction,

[19] Walter Alvarez, The metabolic gradient underlying peristalsis, *J. Amer. Med. Assn.*, 73, 1919, 1438.

they may actually reach a point when the 'gap' variable is really not being reduced with respect to the circumference of the circle, but actually may be enlarging. It seems clear then that without adequate control of some of the features of the reproduced figures one cannot be at all certain that all the characteristics are undergoing change in the same direction during the total time period.

On the other hand, in the methods of successive comparison and recognition as studied in this laboratory, the nature of the changes are subject to control, since only one characteristic aspect of a given figure was allowed to undergo change. The size for instance of figures was in all cases held constant, so that any change that did occur had to take place with reference to degree of angulation or 'gap' space, etc. Thus we are more certain of the direction of change in the given characteristic than are those using the method of reproduction.

The reason for the reversals which take place can at the present moment only be suggested. The results obtained by Gibson[20] and more especially Carmichael and his students[21] suggests the importance that verbal response of *S* to the stimulus-forms may have on the latter responses. We offer the suggestion that such verbal responses not only have an immediate effect but have a protracted effect and serve as one factor in limiting the degree of change which may occur. Thus, suppose an *S* is exposed to an angle which he verbalizes as an "obtuse angle," it is held that no matter what change might occur in that *S*'s memorial apparatus as the result of time he will tend to retain either in the reproduction or in the recognition of forms the characteristics of 'angularity' and 'obtuseness.' In a similar manner if he sees a stimulus-figure as a "circle with gap," it seems unlikely that it will ever lose the characteristics of being 'circular' nor will it loose the characteristics of having a 'gap.' This is a very significant problem and as yet no evidence has been obtained which will substantiate any point-of-view on it. It is hoped that this may be settled as the result of a study which is now being planned.

It is very likely that other factors than verbalization serve to limit the amount and direction of change which can take place in the memorial mechanism either for figures which are to be reproduced or recalled for purposes of recognition and comparison. Such factors as judgment, training, etc., may well take part in the limitations placed upon change and in the reversal effect.

It is suggested therefore, that there are numerous limiting factors in-

[20] *Op cit.*
[21] *Op. cit.*

volved in the direction of change and probably in the rate of change. These factors act upon the "memorial gradient" suggested by Koffka[22] and others in very much the same manner as an ulcer is presumed to act upon the gastro-intestinal tract—in short, producing a reversal of gradient. These factors are *local irritants* and when they are stimulated by a tendency in the dynamic process of change, which is in excess of their threshold, they tend to not merely stop the change but to reverse it. This is by no means intended as an absolute statement of the possible mechanism involved in these apparent reversals, but merely an attempt to indicate a possible process by which such results might occur in a dynamic system as is posited for the memorial apparatus.

Let us consider other examples in order to make the "reversible gradient hypothesis" more comprehensible. During the course of this study, the experimental group tended during the first three trials (up to the seventh day) to 'widen the gap' of the triangular figure, yet by the 28th day they had reversed this and begun to 'close the gap.' It is suggested that here the dynamic tendency on the part of the memorial mechanism involved was toward "increasing the size of the gap" and that this continued until a point was reached when *S* consciously realized what he was doing and then he deliberately attempted to compensate for this with the resulting reversal—this conscious reaction might have occurred any time after the trial on the 7th day and before the trial on the 28th day. It is possible that by the time *S* reached the 28th day he was not even conscious of this reaction. Interestingly enough, while no systematic introspective accounts were taken during this experiment, numerous inquiries were made of the *S*s at random and no such conscious reactions were reported. The *S*s used, however, were untrained in introspective techniques and one cannot place too much reliance upon the statements in this regard. It is hoped that opportunity for checking the possible conscious content involved in the memorial changes will be investigated.

Whether the verbal factor, the judgment, etc., actually rise to the conscious level or not it does seem likely that they impose a definite effect which served to prevent the triangular figure from going too far along the path of change toward becoming either a "rectangle with three sides" or a "triangle."

In a similar manner the "acute angle" showed a tendency toward "closure," yet when this had advanced beyond a certain point a definite and clearcut reversal occurred.

It is interesting to note that reversal is apparently not a reaction to too rapid rate of change, for reversal in some instances occurred when only a

[22] *Op. cit.*

moderate change occurred previously, and in other instances even after rapid change, reversal did not occur.

One of the best tests of the "reversible gradient" effect should be the demonstration of a "rhythmical tendency toward reversal." If there are certain factors which serve as a check and balance in a dynamic system we would anticipate that studies made of the memorial changes by any controlled method (in which only a single variable is studied in each figure) we should find the change proceeding in one direction for a certain time, reversal occurring when the threshold of the limiting factors have been reached, once reversed the change should proceed in the new direction until once again limiting factors at the other end of the memorial gradient become irritated and the balance is again shifted. This method of checking the hypothesis will require memory studies to go on over long periods, but the end-result should be significant.

Conclusions

(1) A method of recognition has been presented which is designed to show in quantitative terms the effect of time on the recognition of visually perceived forms.

(2) The obtuse angle of 120° undergoes a constant change in the direction of becoming more obtuse at the average rate of about 3.15% of its total angularity for each of the 4 trials covering the periods of 3 min., 10 min., 7 days, and 28 days after exposure. The greatest change taking place on the 7th day (3.62%) and the least on the 28th day (2.18%).

(3) The acute angle of 35° undergoes a change but not quite as constant in direction as the obtuse angle. Its average rate of change per trial was found to be 4% of the total angulation with the maximum change being found on the 7th day (—7.03%) and the least, on the trial 10 min. after the exposure to the stimulus-figure (—0.63%).

(4) The circle with gap showed a constant change in the size of the gap, and always in the direction of decrease in the size of the gap. The average rate of change per trial was 6.19%, with the greatest change being found on the 28th day (—11.02%) and the least on the 10-min. trial (1.14%).

(5) The triangle with gap showed less regularity in the direction of change, tending first toward a widening of the gap, then later reversing toward a closure of the gap. The average change per trial was 2.69% with the maximal change occurring at the 10-min. trial (3.45%) and the least being 2.00% at the 3-min. trial—both of these being toward widening of the gap.

(6) The dot figure showed the least rapidity in rate of change although

the change was fairly constant in direction. The average rate of change was only 1.35% of the distance between the fixed dot in the lower left corner and the variable dot in the upper right corner. The greatest change occurred at the 10-min. trial (2.64%) and the least on the 28th day (—0.13%).

(7) Constant change is being undergone in the recognition of simple geometric figures. The change varying in direction and rate with the individual figure.

(8) The same tendency toward asymmetry of judgment previously pointed out as a "time-error-like" phenomenon occuring in the method of successive comparisons over long time-periods, was also found to be definitely present in the judgments made by the method of recognition.

(9) A hypothesis applicable to the nature of the reversals in the direction of change found in the methods of successive comparison, in learning visually perceived forms, and in the method of recognition has been suggested. This hypothesis suggests that substituting the concept of a "reversible gradient" for the suggested "gradient hypothesis" of memorial change indicated by Koffka, Perkins, and others would explain the dual-directional changes found. The nature of the reversal, the time of its occurrence, etc., being controlled by various limiting factors such as verbalization, judgment, and the nature of the figure. These limiting factors are assumed to have a threshold of activity, which, if exceeded results in a rise in the "memorial gradient" in a point of ordinarily low sensitivity and thus producing a reversal.

(10) Methods of testing this hypothesis are suggested and the need for developing this or some principle that will explain the nature of the results is stressed.

(11) The suggested effect of variability of judgment based upon the assumption that *Ss* deviate in their judgments even when the stimulus-figures are present for direct comparison was tested. In general this was found to be of little or no consequence in any of the first trials (since it was of greatest importance for control purposes in the 3-min. trial). Only the circle with gap wielded results that showed no difference between the means obtained by direct comparison and by the recognition-method. This may be discounted, however, in light that in the first trial of this figure, those *Ss* who did not select the identical figure with reference to the stimulus-figure (recognition-method) tended to select figures in a chance distribution, the phenomenon also occurred in the selection by direct comparison.

THE EFFECT OF PATTERN UPON SIMULTANEOUS LETTER-SPAN

By HERBERT WOODROW, University of Illinois

The porportion of the letters correctly named by a given subject when the letters are exposed tachistoscopically for 0.1 sec., a measure of what is here termed simultaneous letter-span, is a function of many external conditions.[1] These conditions are here regarded as conditions determining the difficulty of the test-performance, or, what amounts to the same thing, as conditions determining the goodness of performance (*i.e.* the proportion of letters named correctly).

The quantitative formulation of the interrelationship of the various conditions of goodness of score is a problem which confronts the psychologist in the case of every task-performance. The choice of simultaneous letter-span for a pioneer study of this problem was dictated not so much by special interest in this particular matter as by the consideration that many of the numerous external conditions of the task-performance required of the subject could be readily controlled. As a result of the investigation of the interrelationship between number of letters simultaneously presented and spatial separation, a general formulation concerning the interrelationship of those two conditions was reached. The proportion of letters named correctly under any set of external conditions was first transformed into an "*x*-score"—an *x*-score being the sigma value of a percentage, that is, the distance on the base-line of a normal distribution surface, from the mean, to the point at which a perpendicular would mark off a percentage of the area identical with the percentage constituting the score in question. It was then found that a change in the number of letters simultaneously presented from 2 to 4 had a constant effect upon the *x*-scores at all spatial separations. In this study, however, whenever 4 letters were presented, they were presented evenly spaced, on a horizontal line, and placed symmetrically with respect to the fixation-point. Only with such even spacing did it seem reasonable to suppose that the pattern could be considered to remain constant in spite of increase in spatial separation. Four letters may just as readily, however, be presented in any of an almost infinite variety of patterns. A study of this complex matter of the effect of pattern was therefore indicated, and a simple beginning has been made with the use of 7 patterns of 4 letters each.

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[1] This is the fifth of a series of investigations concerning the relation of the goodness of a performance to the total constellation of its conditions. For preceding studies, see the following: The measurement of difficulty, *Psychol. Rev.*, 43, 1936, 341-365; The interrelation of conditions of difficulty: I. The effect of changes in number at various spatial separations on simultaneous letter span, *J. Gen. Psychol.*, 16, 1937, 83-102; and II. Number, spatial separation and illumination as conditions of simultaneous letter span, *ibid.*, 103-130; and Two quantitative laws relating to goodness of performance, *J. Psychol.*, 4, 1937, 139-159. Other studies will follow.

The seven arrangements of letters on the stimulus-cards and the designation which will hereafter be applied to each of these arrangements is indicated by Fig. 1. It should be understood that in all the patterns the letters were placed symmetrically with respect to the fixation-point.[2]

The patterns are obviously of three types. In the X patterns, the separation of the inner pair of letters remained unchanged at 0.67 cm., while that of the outer pair was changed from 6 cm. in the X_6 pattern to 12 cm. in the X_{12} pattern. In the Y patterns, the letters were evenly spaced at all separations, so that the relative separations remained constant in Y_2, Y_6, and Y_{12}. When all the letters are arranged symmetrically with respect to the fixation-point on the same horizontal line, as in the present study, pattern would seem to become simply a matter of the *relative* separation of any two of the letters, compared with that of any other two. By this definition, pattern is constant in the three Y patterns. In the Z patterns, the separation of the inner pair of letters was always 0.67 cm. less than that of the

DESIGNATION	EXPOSED PATTERNS	DISTANCES	
		M-T	K-S
X_6	M K S T	6	2/3
X_{12}	M K S T	12	2/3
Y_2	M K S T	2	2/3
Y_6	M K S T	6	2
Y_{12}	M K S T	12	4
Z_6	M K S T	6	4 2/3
Z_{12}	M K S T	12	10 2/3

FIG. 1. SHOWING THE RELATIVE SEPARATION OF THE LETTERS IN THE VARIOUS SPATIAL PATTERNS INVESTIGATED

The distance M-T is the distance, in cm., between whatever two letters constituted the outermost pair and the distance K-S that between whatever two letters constituted the inner pair.

outer pair. Consequently in the Z_{12} pattern the two letters at the left and also the two at the right were *relatively* closer to each other than in the Z_6 pattern. The Y_2 pattern might just as well be called a Z_2 or an X_2 pattern, since all three types of patterns, as here conceived, would become identical when the separation of the outer pair of letters was only 2 cm. As regards the distribution of attention which the patterns appear to call for, they might be characterized as follows: the X patterns seem to call for attention at three regions on the exposure card; namely, the fixation-point (near which were two letters) and two other points considerably to the left and right, respectively; the Z patterns seem to call for attention at two

[2] The separation of the letters may be readily expressed in angular measures instead of cm. by the aid of the distance between the fixation point and the bridge of *S*'s nose, a distance of 70 cm. The tangent of ½ the angle subtended by any two letters symmetrically placed with respect to the fixation-point is therefore ½ their separation measured in cm. divided by 70.

regions, lying on opposite sides of the fixation-point; and the Y patterns seem to call for a rather even distribution of attention. The position of the retinal image of the inner pair of letters was always most favorable in pattern X and least favorable in pattern Z, for any given distance between the outer pair of letters.

Of course, in addition to variation in the spatial pattern, the various total constellations of conditions represented by X_0, X_{12}, Z_0, Z_{12}, Y_2, Y_6, and Y_{12} varied in other ways. It is impossible to vary pattern as such without at the same time introducing a change in the retinal positions stimulated. As has been well established, the sensitivity to form is much greater near the fovea than towards the periphery.

Object

The purpose of this study is not simply one of determining the effect of the various total constellations of conditions upon the proportion of letters named correctly, but the more difficult one of ascertaining the relative favorableness of the various patterns as such, apart from, and in addition to, the difficulty due to more or less favorable position of the retinal images of the letters. The aim, in other words, was to determine the effect of pattern after correcting, or allowing for, the effect of the particular locations of the individual letters.

Method and Procedure

The allowance for the effect of location was accomplished by regarding all the 4-letter patterns as composed of two pairs of symmetrical 2-letter patterns. Now the difficulty of single pairs of letters, at various separations, may be readily determined experimentally. The raw scores for such determinations will consist of the percentages of the exposed letters which are correctly named. The average of the percentages correct for two pairs of different separation does not, of course, give the percentage score which should be expected when those two pairs are presented simultaneously, or as one pattern of 4 letters. Such an average does, however, give the percentage-score which should be expected if combining the pairs of letters into a single pattern had no effect upon difficulty. It follows that the variation in difficulty of the various patterns, as such, may be determined by noting the variation in the increases in difficulty of the patterns beyond the difficulty represented by the average percentage correct of their component pairs. It is precisely this method which has been used. Theoretically, it would be possible to regard the 4-letter patterns as composed of four single letters occupying various spatial locations, and then to study the variation in the increases in difficulty shown by the patterns beyond the average difficulty of the 4 isolated letters. Some experimental investigation was made of this method, but it appeared to be unsatisfactory, partly because of the practically perfect scores which were obtained when single letters were exposed, and partly because of the incentive to *S* to allow his fixation to wander from the fixation-point. At best, the procedure seems to offer no advantage over that which has been used. The effect of location is sufficiently determined by studying the percentages right in the case of pairs of letters. Thus, while two letters presented simultanously constitute a more difficult condition than do single letters, that is,

while the scores are influenced by the condition of number, the effect of location or the position of the retinal image is also fully included as a determinant of difficulty.

When we pass from 2 to 4 letters, the increase in difficulty may be regarded as due to two factors, and two only. One is the effect of the larger number of letters —four in place of two. But this factor is constant in all 7 patterns. Since 4-ness is a more difficult condition than 2-ness, the percentage right for two pairs presented simultaneously, that is, for a 4-letter pattern, will always be lower than the average percentage right for the individual component pairs. The other factor is pattern. The *decrease* in the percentage correct, then, from the average percentage correct with two separate pairs to the percentage correct in the case of the 4-pattern made by combining these pairs, is due both to pattern and to 4-ness. Since, however, 4-ness is a constant factor in all 4-letter patterns, differences in the amount of this decrease in the case of different patterns must be due to differences in the effect of pattern alone. What the data to be presented show, is not the absolute effect upon difficulty of any pattern, as separate from its 4-ness, but the *variation* in difficulty due solely to variation in pattern, after eliminating the effect of number (or 4-ness) by keeping it constant, and after also eliminating the effect of spatial position by correcting for it. The effect of spatial position, as already explained, is present in full as a determinant of the difficulty of single pairs. The allowance or correction for this factor, then, is accomplished by subtracting from the difficulty of any set of four letters the difficulty to be expected were the set of four letters identical with two pairs of letters presented separately. In order to make this subtraction it is of course necessary to determine the percentage right in the case of the various pairs required to duplicate the seven used 4-letter distributions. Seven such separations of pairs of letters are required, to duplicate the seven X, Y, and Z 4-letter patterns already described. They are as follows, in cm.: 0.67; 2.0; 4.0; 4.67; 6.0; 10.67; and 12. Two of these separations, the 4 and the 4 2/3 cm., are so close together that it was decided to calculate the results for the former, *i.e.* the 4 cm. separation, by interpolation, and to omit it from the experimental series. The experiment therefore was conducted by exposing sets of 2-letters of 6 different separations, and sets of 4-letters of 7 different spatial arrangements, making a total of 13 varieties of exposure cards.

Six sets of exposure cards, 10 in each set, were used in the case of each of these 13 letter-arrangements. Two of the sets were used in the 2 hours of fore-practice given every *S*, and the remaining 4 sets constituted the experimental series. *S* was always informed as to the arrangement of letters to be presented, and any one arrangement was always presented 10 times in succession (though no two cards in a set were identical). The 13 arrangements, or varieties of sets, were used in a carefully counterbalanced order, following the A B B A principle. With 12 *S*s, the 2-letter sets were used on the 3rd and 6th days and the 4-letter sets on the 4th and 5th days. This order was reversed with the remaining 11 *S*s (originally 12, but one was forced to discontinue), that is, the 3rd and 6th days were devoted to 4-letter sets and the 4th and 5th to 2-letter sets. The letters were Willson's black gummed, capital letters, 3/8 in. in height, and exposed fastened to a rectangular strip of white egg-shell paper 20 cm. long and 4 cm. high. The experiment was conducted in an artificially illuminated dark room. The illumination, the make-up of the letter series,

and the arrangement of the experiment in all its details were identical with those described in a previous paper.[3]

Treatment of the Data

At the conclusion of the experiment, the average proportion correct for each arrangement of letters was determined. Then in the case of 2-letter sets, the averages for the two separations required to reproduce each of the 4-letter separations was calculated. The required separations are included in the tables of results. Thus the 4-letter pattern X_6 may be analyzed into a combination of 2 letters separated 0.67 cm. and 2 letters separated 6 cm., both 2-letter patterns being symmetrical with respect to the fixation point. The average proportion of the 2-letter sets correct with a separation of 0.67 cm. was 0.968 and with a separation of 6 cm. was 0.871. Now, if 4 letters, simultaneously presented, were no harder than 2 letters, that is, if number of letters simultaneously exposed were not a condition of the performance, then, when the two sets of two letters, one with a separation of 0.67 cm. and one with a separation of 6 cm. were presented simultaneously, the situation would be the same as if the 4-letter X_6 pattern were used. If the fact of 4-ness were inconsequential, then the proportion correct should be the average of the two proportions given above for the sets of two letters; namely, 0.968 at 0.67 cm. and 0.871 at 6 cm. The average of these two proportions is 0.920. The proportion correct for the 4-letter X_6 pattern, however, was only 0.742. This lowering in the proportion correct from 0.920 to 0.742 is therefore due to the greater difficulty of 4 letters than of 2 letters, when the 4 letters are presented in the pattern X_6. To determine this increment in difficulty when the 4 letters are presented in some other pattern, it is necessary to observe what two 2-letter patterns reproduce the 4-letter pattern and then to proceed in the same way. Thus the two 2-letter patterns which reproduce the Z_6 4-letter pattern are those of 2-letters separated by $4\frac{2}{3}$ and by 6 cm. The average proportion correct at $4\frac{2}{3}$ cm. was 0.929 and at 6 cm., 0.871. The average of these two proportions is 0.900, whereas the average proportion correct with Z_6 was only 0.613. Thus, 4-ness patterned as Z_6 caused an increment in difficulty represented by the change in the proportion correct from 0.900 to 0.613, or a change of 0.287, while 4-ness patterned as X_6 caused an increment in difficulty represented by the drop from 0.920 to 0.742 percent, or a change of 0.178. The difference in these two increments in difficulty may be attributed to a difference in pattern, since X_6 and Z_6 are alike as regards the number condition, *i.e.* they are both 4-letter patterns. Obviously the drop of 0.287 from 0.900 to 0.613 (Z_6) is greater than that of 0.178 from 0.920 to 0.742 (X_6). It follows that the pattern Z_6, as such, *i.e.* apart from the effect of number, and apart from the effect of position of the retinal images of the letters, was more difficult than the pattern X_6. Just how much more difficult, however, is hard to determine exactly as long as the effect is expressed in terms of a change in percentages correct, particularly in cases where it is necessary to compare drops in two widely different percentages. For this reason, all proportions correct have been expressed as x-values, and all changes calculated as differences in x-values.

It is believed that these differences in x-values are comparable at all values, and

[3] The interrelation of conditions of difficulty, *J. Gen. Psychol.*, 16, 1937, 85-88.

closely reflect all the corresponding changes in difficulty in terms of a constant unit. Their use involves the assumption that the mean percentage correct under various constellations of conditions varies inversely with difficulty (or directly with absolute goodness of score) in accordance with a normal distribution area. Any percentage-score is therefore regarded as representable by a corresponding proportion of a normal distribution surface. The difficulty of that constellation of conditions under which the score was obtained may be taken as the distance along the base-line of the surface which must be laid off from the mean to reach the point at which a perpendicular marks off a percentage of the surface identical with the percentage-score. The base-line distances thus obtained, *i.e.* the *x*-values,[4] which are used as absolute measures of difficulty, were found to agree quite closely with values obtained by Thurstone's method of absolute scaling. Whatever may be the inaccuracy in the assumptions involved in the use of *x*-scores, and it is unlikely that they are more than approximately correct, such scores are certainly far superior to mere changes in percentage. Their use magnifies the relative significance of percentage changes near 100 or 0 compared with equal percentage changes occurring near 50. For example, expressed in terms of *x*-values, a change from 90 to 95% represents as great a change in difficulty as one from 50 to 64%.

Results

The results obtained with the group of 23 *Ss* are given in Table I. The total number of letters from which each proportion correct (p) is calculated is 3680, represented by 4 sets of 10 exposures each, given 23 *Ss*, *i.e.* by $4 \times 10 \times 23$, or 920, trials. The total number of letters exposed, then, in the case of each 4-letter pattern was 4×920, or 3680. Each 2-letter percentage was also based on 920 trials; but since two 2-letter percentages are averaged together for comparison with each 4-letter pattern, the total number of letters for each combination of 2-letter patterns given in Table I is also 3680. The table shows the mean proportion of letters correct in all cases except that of the separate 2-letter series. The proportions correct for these series, each basd on 1840 letters, were as follows: 0.67 cm., 0.968; 2 cm., 0.964; 4 cm., 0.938 (by linear interpolation); 4.67 cm., 0.929; 6 cm., 0.871; 10.67 cm., 0.654; 12 cm., 0.572. The most important columns of Table I for the purpose of the present study are those under the heading, Diff. (A-B), *i.e.* the difference between the score for a 4-letter pattern and the average for the duplicating pair of 2-letter patterns. While the table shows the differences in the proportions of letters correct[5] as well as the differences in the *x*-values, only the latter

[4] Woodrow, The measurement of difficulty, *loc. cit.* See also, The interrelationship of conditions of difficulty, *J. Gen. Psychol.*, 16, 1937, 92 ff.

[5] Perhaps all the observed proportions should be corrected for chance. The *Ss* were instructed to guess if they could, but a guess was not required and many responses were "blank." It is impossible, therefore, to determine the proper correction for chance successes. However, all the values given in Table I have been

are to be considered comparable with each other. The greater the x-value difference in this column, the greater the difficulty of the 4-letter pattern listed on the corresponding line in the extreme left-hand column. These differences between the x-values of a 4-letter pattern and the duplicating pair of 2-letter patterns will hereafter, for convenience, be termed the 4-letter effects. They represent the increase in difficulty of 4-letters patterned in a particular way over the difficulty of the corresponding pair of two letters. They are measures of the effect of 4-ness, as compared with 2-ness, with the factor of retinal position eliminated. Only the differences between these differences, that is, the differences in the 4-letter effects, are due solely to pattern. Differences between the 4-letter effects will be termed pure pattern effects.

TABLE I

DIFFERENCES BETWEEN FOUR-LETTER AND CORRESPONDING TWO-LETTER PATTERNS

(Every p is the proportion correct of 3680 letters)

(A) 4-letter patterns			(B) Two 2-letter patterns			Diff. (A−B)	
Pattern	p	x	Separations	p	x	p	x
X_6	.742	− .650	3 and 6	.920	−1.405	.178	.755
X_{12}	.544	− .111	3 and 12	.770	− .739	.226	.628
Y_2	.856	−1.063	3 and 2	.966	−1.825	.110	.762
Y_6	.725	− .598	2 and 6	.918	−1.392	.193	.794
Y_{12}	.463	+ .093	4 and 12	.755	− .690	.292	.783
Z_6	.613	− .287	4½ and 6	.900	−1.282	.287	.995
Z_{12}	.301	+ .522	10½ and 12	.613	− .287	.312	.809

It will be readily observed that the wide differences in difficulty due to the 4-letter constellations taken as a whole largely vanish when correction is made, in the manner described above, for the effect of the retinal positions stimulated. For example, the uncorrected or total difficulty, measured in terms of x-values, was only —0.598 for Y_6 but +0.093 for Y_{12}, a difference of 0.691. When the Y_6 and Y_{12} x-value scores are both corrected for spatial position, by substracting from them the x-value of the average proportion correct with the duplicating pair of 2-letters, that is, when the score is made to reflect the effect of 4-ness as such, this difference between Y_6 and Y_{12} disappears, the 4-ness effect obtained in the case of pattern Y_6 being 0.794 and in that of pattern Y_{12}, 0.783.

computed over again, by Guilford's formula (*Psychometrika*, 1, 1936, 261), on the assumption that, since there are 26 letters in the alphabet, 1/25 of the responses were correct solely by chance. The resulting (A-B) differences, or 4-letter x effects were as follows: X_6, 0.760; X_{12}, 0.645; Y_2, 0.776; Y_6, 0.802; Y_{12}, 0.805; Z_6, 1.016; and Z_{12}, 0.858. In general, the conclusions regarding the difficulty due to pattern *per se* which would be drawn from these values are identical with those based on those presented in Table I.

For the most part, the differences in the effect of 4-ness due to the various patterns seem to be small. It is desirable, therefore, to determine whether they exceed the differences which might result from errors of sampling. Two sorts of sampling are involved: first, the observed responses of these *S*s are but a sample of the responses of these particular *S*s; and secondly, the 23 *S*s are a sample of some larger, inexactly defined, population of *S*s. There are thus two distinct questions: first, whether pattern had a significant effect upon these particular *S*s; and secondly, whether it would be safe to predict that the effects observed with these particular *S*s would also be obtained with another group of *S*s selected from the same general population. As a guide towards answering the first question, the data with each type of exposure series was fractionated into fourths. The *x*-value

TABLE II

FOUR-LETTER *x*-VALUE EFFECTS FOR SUCCESSIVE FOURTHS OF THE EXPERIMENT

Portion	X_6	X_{12}	Y_2	Y_6	Y_{12}	Z_6	Z_{12}
1st fourth	.777	.711	.853	.698	.823	.864	.710
2d fourth	.804	.552	.733	.834	.820	.976	.772
3d fourth	.658	.643	.711	.723	.770	1.140	.949
4th fourth	.796	.610	.788	.967	.721	1.053	.814
Mean	.759	.629	.771	.805	.784	1.008	.811

differences of each pattern were then calculated four times, the calculations being based on successive fourths of the data. Thus, using the first fourth of the data on the 6 cm. and 2/3 cm. 2-letter series, a mean proportion correct of 0.903 was obtained. The first fourth of the data on the 4-letter pattern X_6 (*i.e.* first 10 trials, 23 *S*s) gave a mean proportion correct of 0.699. The *x*-values of these two proportions are —1.299 and —0.522, and therefore the *x*-value difference for this portion of the data was 0.777. Each of the remaining fourths of the data was treated in the same manner, so that three additional 4-letter *x*-value effects were obtained for pattern X_6. Now if the differences in the mean 4-letter *x*-value effects of any two different patterns is significant,[6] the four separate values for the one pattern should not greatly overlap those for the other pattern. A plausible, though crude, criterion of significance in the present instance, of difference in 4-letter effect due to pattern, is that not more than one of the *x*-value 4-letter effects of one pattern should overlap the range of *x*-value 4-letter

[6] The mean 4-letter *x*-value effect given in Table II in no case agrees exactly with the corresponding value in Table I because the mean of the *x*-values of four proportions is not ordinarily identical with the *x*-value of the mean of those four proportions.

effects of the other. The data necessary for making these estimates of significance are given in Table II.

If the criterion of reliability mentioned above is applied to the data of Table II, it is clear that there is no reliable difference in the difficulty due to 4-ness of the patterns X_6, Y_2, Y_6, Y_{12} and Z_{12}. The various x-value 4-letter effects in the case of Y_{12}, where the range of these values is smallest, vary from 0.721 to 0.823 inclusive. The number of 4-letter x-value effects of the other 4-letter patterns falling within this range is as follows: X_6, 3; Z_{12}, 2; Y_2, 2; and Y_6, 1. Pattern Y_{12} cannot be considered more difficult than Y_6 because *all* of the Y_{12} values fall within the range covered by those for Y_6. It is also true that all four of the Y_{12} values fall within the range of the Y_2 values, *i.e.* 0.711 to 0.853. It is improbable then, that any significant difference in the effect of pattern *per se* occurred in the case of patterns X_6, Y_2, Y_6, Y_{12}, and Z_{12}. On the other hand, in the case of Z_6 only the lowest value (namely, 0.864) falls within the range of those for any other pattern. Pattern Z_6, then, may be regarded as significantly more difficult than any of the others. And only the largest of the X_{12} values, 0.711, exceeds the lowest of any other pattern. Pattern X_{12}, then, was significantly more difficult for these *S*s than any other pattern.

TABLE III

MEANS OF THE RESULTS OBTAINED WITH INDIVIDUAL *S*s

(A) 4-letter patterns			(B) Two 2-letter patterns			Difference (A−B)	
Pattern	mean x	σ_m	Separations	mean x	σ_m	Mean x-diff.	$\sigma_{m\ x\text{-}diff.}$
X_6	−.688	.063	$\frac{3}{8}$ and 6	−1.481	.069	.793	.062
X_{12}	−.122	.042	$\frac{3}{8}$ and 12	− .751	.073	.629	.068
Y_6	−.640	.063	2 and 6	−1.443	.077	.803	.070
Y_{12}	+.092	.035	4 and 12	− .672	.050	.765	.040
Z_6	−.287	.054	$4\frac{3}{8}$ and 6	−1.323	.042	1.036	.054
Z_{12}	+.530	.046	$10\frac{3}{8}$ and 12	− .294	.042	.824	.037

The results so far considered are those obtained by pooling the responses of all 23 *S*s. The 4-letter x-value effects have also been calculated for every *S* separately. The 23 x-value differences thus obtained in the case of each 4-letter pattern were averaged, and the sigma of the average calculated directly from the 23 values of the difference. From these sigmas it is possible to determine whether the differences due to pattern in the 4-letter effects are significant in the sense that they should be expected in any similarly selected group. Since the x-value of the mean of 23 proportions will ordinarily differ from the mean of the 23 x-values of those propor-

tions, the 4-letter effects as determined by averaging the 23 individual x-values will differ slightly from those given in Table I, where the x-values are calculated from the proportions correct shown by the group as a whole. In calculating the x-values for individuals, the Y_2 pattern had to be omitted, because 2 *Ss* obtained perfect scores in both of the pairs of duplicating 2-letter patterns, *i.e.* 2-letters separated by 0.67 cm. and by 2 cm. The means of the individual x-values and the means of the individual 4-letter x-effects, together with their sigmas are shown in Table III.[7]

According to Table III the differences in the various mean 4-letter effects are slight compared to the individual differences in the effect of each pattern. The variation in difficulty due to variation in pattern, however, closely parallels that shown by Table II. Pattern Z_6, as before, is significantly more difficult than any other pattern, with the possible exception of Y_6, in which case the critical ratio is only 2.6 (all CR's being here based on the sigma of the difference between the two mean 4-letter x-effects). Pattern X_{12} again is found to cause the least difficulty, though it is significantly easier only when compared with Z_6 (CR = 4.7) and possibly with Z_{12} (CR = 2.5). As before, there appears to be no significant difference between X_6, Y_6, Y_{12}, and Z_{12}. The order of difficulty of the various patterns then remains the same as that indicated by Table II, except for a faint suggestion that Z_{12} has a degree of difficulty intermediate between the most difficult, Z_6, and the next lower level of difficulty, namely, that presented by patterns X_6, Y_6, and Y_{12}. According to both tables, pattern X_{12} is the least difficult.

It is obvious that the difficulty due to pattern as such does not at all parallel the difficulty due to the total constellation of conditions. For example, as total constellations, Z_{12} and X_{12} are far more difficult, respectively, than Z_6 and X_6; but the most important cause of this fact lies not in the effect of pattern, but in the distance from the fovea of the retinal positions stimulated, a distance which is much greater in the 12 cm. than in the 6 cm. patterns. The data of Tables II and III, however, establish the fact that the pattern X_6, as such, is a more difficult one than X_{12}, and that the pattern Z_6 is a more difficult one than Z_{12}.

This greater difficulty on the part of patterns either of the X type or of the Z type at 6 cm. than at 12 cm. is somewhat surprising. It suggests a principle to the effect that the greater the separation of the letters and

[7] It will be observed that the σ of the mean difference (A-B) in x-values does not agree with the formula $(\sigma_{m_A}^2 + \sigma_{m_B}^2)^{\frac{1}{2}}$, but has a smaller value due to correlation in the 23-*Ss* between the 2-letter scores and the 4-letter scores. The degree of such correlation may be readily estimated from the formula $r_{AB} = (\sigma_A^2 + \sigma_B^2 - \sigma_d^2)/(2\,\sigma_1\,\sigma_2)$. It varies from +0.389 to +0.671.

possibly, also, the less their visibility on account of distance from the fovea, the less is the effect of 4-ness as such in any pattern. Do letters close together and distinctly seen letters cause greater confusion, or more mutual inhibition in the process of being named, than widely separated or poorly seen letters? The observed *x*-values vary as though this were the case, for in both the X and Z type of pattern, at 12 cm. as compared with 6 cm. the average separation of the letters was greater and the average visibility of the letters less, the visibility being less because of a more peripheral location of the retinal images of the letters (of two of the letters in the X_{12} pattern and of all of the letters in the Z_{12} pattern). The effect of 4-ness was less in both patterns at 12 cm. than at 6 cm. A similar principle was suggested by the results of a previous study in which it was found that the decrement in goodness of performance produced by presenting six letters simultaneously in place of two letters was less when the letters were dimly illuminated than when brightly illuminated.[8] Now it is true that the uncorrected *x*-values of the 4-letter patterns (*i.e.* the values given in the tables under Section A), like the proportions correct, are different for the 12 cm. than for the 6 cm. separations. This is also true of the *x*-values of the average proportion correct for the duplicating pairs of two-letters (given in Section B). It might, therefore, be suspected that differences in *x*-values, like differences in proportions, are not comparable when each difference is reckoned from a different level. While it should probably be conceded that this is a possibility, the evidence against it is strong. The data obtained with the Y patterns, for example, show no tendency for the *x*-value differences due to 4-ness to decrease, as a result of some statistical artifact, with increase in the level of difficulty at which the difference is calculated. The data taken as a whole, then, show no general tendency for the *x*-value differences either to decrease or increase with the level at which they are calculated. Moreover, these *x*-values,[9] based on proportions correct, were found to agree with scaled scores when scaled on the basis of the proportion of individual *Ss* (in a group of 168) exceeding the various scores.

If the hypothesis that the effect of 4-ness is lessened by increased separation and lessened visibility of the letters be true, why, it may be asked, is not the Y_{12} 4-letter effect less than that of Y_6? In the former the letters are more widely separated and on the average less visible. Evidently a third influence complicates the situation. This may very well be a greater difficulty of dividing attention so as to concentrate on widely separated letters or

[8] The interrelation of conditions of difficulty, *J. Gen. Psychol.*, 16, 1937, 113 ff.
[9] Cf. Woodrow, The measurement of difficulty, *loc. cit.*

groups of letters. Thus, in the Y patterns, where the letters remain evenly spaced at all separations, the greater difficulty of attending to widely spread letters may more or less exactly compensate for the smaller amount of cognitive inhibition with wide-spread letters and letters falling on unfavorable retinal positions. In the case of Z_{12}, however, the greater difficulty due to the necessity of widely divided attention may be supposed to be more than balanced by the otherwise favorable effect of greater separation of the inner letters (as compared with Z_6) and particularly of the much lower average visibility of the letters in Z_{12} than in Z_6. For a similar reason, the pure pattern effect of X_{12} may be less than that of X_6, except than in this case the greater separation and lesser visibility at 12 cm. attaches only to the outer pair of letters. The effect of pattern, then, may be due to a

TABLE IV

EFFECT OF PATTERN WITH LETTERS NAMED IN ORDER FROM LEFT TO RIGHT
(Each proportion p is based on 720 letters)

(A) 4-letter patterns			(B) Two 2-letter patterns			Difference (A−B)	
Pattern	p	x	Separations	p	x	p	x
X_6	.743	− .653	$\frac{3}{4}$ and 6	.911	−1.347	.168	.694
X_{12}	.603	− .261	$\frac{3}{4}$ and 12	.752	− .681	.149	.420
Y_2	.881	−1.180	$\frac{3}{4}$ and 2	.964	−1.799	.083	.619
Y_6	.730	− .613	2 and 6	.906	−1.317	.176	.704
Y_{12}	.532	− .080	4 and 12	.730	− .613	.198	.533
Z_6	.583	− .210	$4\frac{3}{4}$ and 6	.882	−1.185	.299	.975
Z_{12}	.314	+ .485	$10\frac{3}{4}$ and 12	.560	− .151	.246	.636

complex of several factors, among which, it is suggested, are the following three: the spatial closeness of the letters; the visibility of the letters; and the most efficacious distribution of attention. To predict the effect of pattern, as such, it would apparently be necessary to evaluate quantitatively each of these influences. The writer has found it possible to make numerical estimates of each of the three conceivable effects of pattern mentioned above, estimates which seem plausible and which, when summed, gave the same relative pure pattern effects as those actually obtained.

The preceding results were obtained with the *Ss* instructed, in the case of 4-letter series, to name first the outside pair of letters, from left to right, and then the inside pair. These instructions were given in the hope that they would prevent a tendency, suspected in earlier work, for the *Ss* to concentrate upon the naming of the inner pair of letters when the outer pair became very difficult because of wide separation of the letters. The first study made of the effect of pattern, however, was one in which, as in

previous studies, the *Ss* named all the letters in order from left to right. The results obtained by this experiment are shown in Table IV. In addition to the order in which the *Ss* named the letters, the present experiment differed from the one described above in the use of a smaller number of *Ss* and a smaller number of trials. Except for these differences, the experimental situation was identical in detail with that in the case of the 23 *Ss*. The number of *Ss* in the present instance was only 9, all of whom, like the 23 *Ss* used in the previous experiment, were underclassmen with no previous experience with the psychological laboratory. The number of trials given every *S* in the case of each of the 13 types of exposure series was only 20, as compared with 40 in the previous experiment.

Table IV is constructed in the same way as Table I, and summarizes the pooled responses of the 9 *Ss*. A comparison of the two tables, I and IV, indicates that the effect of 4-ness is somewhat less when the 4 letters are named in order from left to right (Table IV) than when the outer letters are named first (Table I). This conclusion is indicated by the fact that the 4-letter effect as measured by the *x*-differences is in all cases smaller in Table IV than in Table I.

In view of the relatively small number of trials and the small number of *Ss* no statistical estimates of the significance of the differences in the 4-letter effects due to differences in pattern have been made. Judging simply from the magnitude of the effects, however, the same three levels of difficulty shown to be significantly different by the data of Tables II and III are indicated by the present data. Thus, pattern Z_6, as before, is by a large margin the most difficult; pattern X_{12} is the least difficult; and all the remaining patterns have a degree of difficulty intermediate between that of Z_6 and X_{12}.

The three Y patterns, however, in the present instance fail to show the highly similar degree of difficulty indicated by the previous data. In particular, pattern Y_6 appears to be definitely more difficult than Y_{12}, the 4-letter *x*-value effect of the former being 0.704 and of the latter, 0.533. This result is not only inconsistent with the data of Table I, but is also inconsistent with the approximate constancy in the 4-ness effect of Y patterns found in two previous studies.[10] The explanation of this inconsistency appears to lie in the fact of individual differences in the distribution of attention in the case of the various patterns. However this may be, it is clearly evident that a given change in external conditions does not produce the same change in internal processes, *i.e.* those going on within *S*, in the

[10] Woodrow, The interrelation of conditions of difficulty, *loc. cit.*

case of all the *Ss*, or even in the case of all small groups of the *Ss*. It is this fact, above all others, which accounts for the slow progress of quantitative psychology, and which, in particular, makes it difficult to establish verifiable quantitative laws concerning the relation of external conditions to goodness of performance. In spite of this difficulty, so far as the two groups here studied are concerned, very considerable agreement has been found in the relative pure pattern effects.

Summary

Determinations were made of the difficulty of naming letters correctly when the letters were presented tachistoscopically, 4 at a time, arranged in one of 7 spatial patterns. Wide variations in the difficulty of these seven 4-letter arrangements largely disappeared when corrected for the effect of differences in the retinal positions stimulated, differences inevitably associated with differences in spatial pattern. There still remained, however, certain significant differences in difficulty which must be attributed to the effect of pattern as such, that is, after allowance both for the effect of retinal position and the effect of number of letters simultaneously exposed. While the explanation of the pure pattern effect is not apparent, it is suggested that it depends upon the entailed distribution of attention and the amount of confusion or cognitive inhibition resulting from the spatial closeness and the degree of visibility of the letters.

THE PRINCIPLE OF CLOSURE IN TERMS OF RECALL AND RECOGNITION

By JOHN JOSEPH TIERNAN, University of Nebraska

According to Gestalt theory as expounded particularly by Koffka and Köhler, the organism, when confronted by a situation which is non-symmetrical, incomplete, out of rhythm—the situation may be an open triangle, an unsolved arithmetical problem, the pangs of hunger—labor under a stress or strain until that disturbing situation is adjusted and equilibrium reached. The final stage when the tension is released and equilibrium reached is known as closure.

The presence in the organism of an inherent force working towards a stable equilibrium is not conceded by such noted psychologists as Pavlov, Watson, and Thorndike. The phenomena which cause tensions are of course recognized by these psychologists, but they are explained by some process essentially different from closure, by Thorndike, for instance, as a "varied reaction guided by the satisfyingness of the results attained."[1]

The real nature of closure has been a fruitful topic for experimentation, particularly in the field of visual perception, since the rise of the Gestalt school. Experiments such as those by Gottschaldt,[2] Wulf,[3] Hector,[4] Gibson,[5] and Fehrer,[6] though furnishing extremely interesting results, have failed to satisfactorily explain the nature of the principle at work. The present writer was consequently moved to make a further study on somewhat similar lines in the hope of getting some definite and possibly measurable estimate of the effect of closure on the processes of recall and recognition.

* Accepted for publication April 8, 1937. This study was directed by Professor D. A. Worcester.

[1] E. L. Thorndike, *Human Learning*, 1931, 125.

[2] K. B. Gottschaldt, Über den Einfluss der Erfahrung auf die Wahrnehmung von Figuren: I. Über die Wirkung gehäufter Einprägung von Figuren auf ihre Sichtbarkeit in umfassenden Konfigurationen, *Psychol. Forsch.*, 8, 1926, 261-317; II. Vergleichende Untersuchungen über die Wirkung figuraler Einprägung und den Einfluss spezifischer Geschehensverläufe auf die Auffassung optischer Komplexe, *ibid.*, 12, 1928, 1-87.

[3] F. Wulf, Über die Veränderung von Vorstellungen (Gedächtnis und Gestalt), *ibid.*, 1, 1922, 333-373.

[4] J. Hector, Les structures mentales, *Rev. de phil.*, 28, 1928, 255-321.

[5] J. J. Gibson, The reproduction of visually perceived forms, *J. Exper. Psychol.*, 12, 1929, 1-39.

[6] E. V. Fehrer, An investigation of the learning of visually perceived forms, this JOURNAL, 47, 1935, 187-221.

Problems

The problems set for investigation were as follows: (1) Are geometric figures which are open or incomplete, more easily recalled than complete ones? (2) In the recall of geometric figures is there a tendency to close gaps, to complete unfinished contours, to readjust disturbed equilibrium, even though one is instructed to reproduce the figures exactly as he saw them? (3) What effect, if any, has the *degree* of incompleteness on the recall of the figure? (4) What effect has the time-interval between perception and recall upon the latter? (5) Will closure be observed more among older than among younger subjects? (6) Has the factor of sex any effect upon the relative recall of open and closed figures? (7) What is the effect of closure upon the process of recognition with regard to each of the above points?

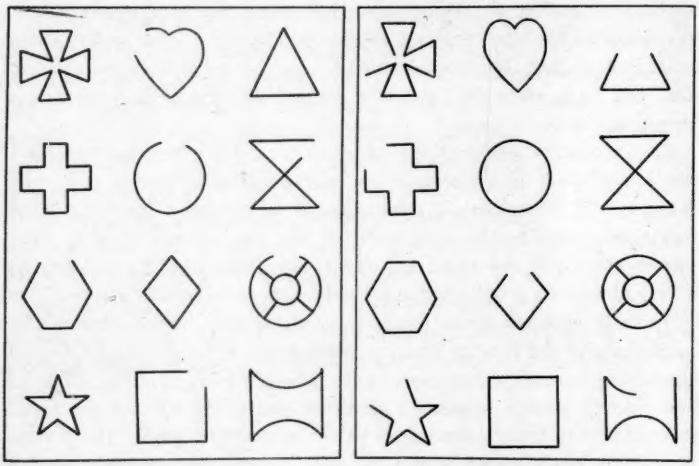

Chart A Chart D

Fig. 1. Samples of the Study Charts Used in the Recall and Recognition Experiments

Recall Experiments

Method and Procedure

Materials. The materials used in the carrying out of the tests, consisted of charts containing simple geometric figures (see Fig. 1). There were 12 figures on each chart: 6 open, that is to say, having their contours incomplete, and 6 closed. The

figures used were such as would be familiar to most of the *Ss*, *e.g.* circle, triangle, square, etc., and all were symmetrical in terms of bisection either perpendicularly or horizontally. Instead of dividing the *Ss* into formal experimental and control groups, a method was devised to control the effect which the nature of the figures themselves might have upon recall—a circle, for instance, might in itself be more easy to recall than a triangle, and this fact, rather than the operation of closure might be the determining factor in the group recall of circles as against triangles. As a control upon this, two groups of charts were devised, the figures which were open on the first group being closed on the second, and vice versa. Thus each group was at the same time an experimental and a control group.

Since one of the objects of the study was to find out the effect upon recall of the different degrees of non-closure, 6 charts in all were prepared, three belonging to each of the groups mentioned above. Every chart had an identification letter: A, B, C, D, E, or F. Chart A contained 6 closed figures, and 6 in the first or smallest degree of non-closure; Chart B had the same 6 figures closed and the same 6 open, but here the non-closure was in the medium or second degree; Chart C had the same 6 closed and the same 6 open, but here the non-closure was in the greatest, or third degree. Charts D, E, and F were constructed on the same principles, except that here, as explained above, those figures were open which were closed on the other three charts, and vice versa. Every geometric figure, therefore, was presented to an equal number of *Ss*, in its closed form, and in each of its various degrees of non-closure. A separate chart was available for each subject who took either the recall or the recognition test.

Subjects. The *Ss* for the recall experiment were 315 grade-school pupils from St. John's parochial school in the city of Omaha. They ranged in chronological age from 7 to 14 yr., in terms of last birthday, and comprised the entire school population from Grades III to VIII inclusive. They were a good sample of the general population at their age levels, their average E.Q. on the Stanford Achievement Test being 111, the range extending from 92 to 126. They were practically all American born, and their home environment in general was that of the middle class.

Procedure. Before the experiment proper was begun, the tests were given to children in another school in order to test out the procedure. The time allowed had to be adjusted to the number and difficulty of the figures, and the capacity of the *Ss*. A period of study which would be sufficient to yield an average immediate recall of 70 or 80% of the whole was deemed suitable. This was tried out before the actual tests upon a dozen or so *Ss* who were fair samples of the groups to be used. A study period of 40 sec. was thus decided upon. The same amount of time was allotted for the junior and senior portions of the groups, both in the recall and recognition experiments. The tests were given to the various grades in the class-rooms during the afternoon class hours. All were given personally by the writer. The *Ss* were not given any hint as to the purpose of the experiment, or the significance of the open and closed figures on the charts. Each class-room population was divided into 6 sections, one section to study each of the recall charts. Care was taken to have an approximately equal number of boys and girls in each section. This made a total of 12 groups, each group having, when the various class-room populations were added together, from 25 to 30 cases. The *Ss* were told that they were going to have a test in memory. The appropriate charts were placed before them, face downwards. The reverse side of the chart was covered by a drawing paper which prevented the outlines of the figures being seen by the *Ss*, who were cautioned not to turn the chart over until the signal was

given to do so. After the charts had been signed, the *S*s were then instructed as follows:

On the other side of the paper which is now on your desk you will see 12 figures. When I give the word I want you to turn the paper over and study the figures. You will have only 40 sec. to do so, so keep looking at the figures all the time. Be sure to study *all* of the figures. I will then ask you to draw on the blank side of the sheet all of the figures that you can remember. If you cannot draw them perfectly it does not matter—just draw them as well as you can, because all I want is to know whether you can remember them. Be sure, however, to try to draw them *exactly* as they are on the paper. No one is to speak during the test, and of course no one is to look at any one else's paper.

The *S*s were given exactly 40 sec. during which they intensively studied the charts. At a given signal they turned the charts over, and proceeded at their leisure to record their recall. The writer is satisfied that there was, if any, a negligible amount of copying during this test. When it was evident by a show of hands that the *S*s were finished, the charts were collected.

The delayed recalls were giver at intervals of 11, 46, and 130 days after the original test. The *S*s were not led to expect that they would be given delayed recalls. On the days decided for these recalls, they were supplied with blank sheets and pencils, and having been reminded of the recall charts they had studied, were simply asked to draw as many of the figures as they could now remember exactly as they were on the chart. They were told that the charts were not all the same, so that it would be worse than futile for anybody to copy. At the conclusion of the recall the sheets were signed.

Scoring. For scoring the tests, printed score-sheets were used, one for every *S*. On the score-sheets and throughout the tests the following terms were used to designate the kind of recall made by an *S*: *open-open* meant an item which was open on the recall chart and was recalled as open by the *S*; *open-closed* meant an item open on the chart, but recalled as closed; *closed-open* meant an item closed on the chart, but recalled as open; and *closed-closed* an item closed on the chart and recalled as closed. In the reproduction of the figures there was very little ambiguity. Even though the drawings might not have been perfect, it was in practically all cases evident which figure *S* intended to recall, and whether he had recalled it as open or as closed. In a few cases the *S*s recorded the figures twice, as open and as closed. In such an event, the one which conformed to the chart the *S* had studied was scored correct; the other was discarded.

The first procedure in the task of handling results was to divide each of the 12 groups into a junior and senior sub-group, the former comprising *S*s of 11-yr. old and under, the latter, *S*s over 11 yr. This was the only division made for the purpose of tabulating results in terms of age. The groups which had studied Charts A and D were then put together; similarly with the groups studying Charts B and E, and Charts C and F. This arrangement resulted in 12 groups, each numbering from 25–30 *S*s.

RESULTS

Two factors were first considered: (1) the amount of *S*'s recall of open figures compared with his recall of closed ones, (2) the fact of closure or non-closure, *i.e.* did S close figures which he saw open on the recall

chart, or open ones which he saw closed? The first factor was studied by comparing the total open recall with the total closed, and it was found that the difference between the means was very slight in all cases. The standard deviations also were so relatively large as to warrant the conclusion that the differences between the groups were not statistically significant. Reliabilities of the differences (diff./σdiff.) were found to be: 0.27, 0.21, and 0.78 for Groups A and D, B and E, and C and F respectively, which means that the chances in 100 of the obtained differences being true differences are 61, 58, and 78 respectively. Though the differences were slight, it was interesting to note, however, that they ran on the whole in the direction of the open figures. These findings are not conclusive. It is conceivable that an experiment set up exclusively to test the power of an open figure, compared with a closed one, to influence recall, might be productive of statistically reliable results.

It was also found that the extent of the interval between recalls had practically no effect on the situation, neither had the degree of non-closure on the charts studied.

Closure. When the total *open-open* recall was compared with the *open-closed,* significant differences were found. In almost all instances Diff./σdiff. = 2.00 or more. It was also found that the *open-open* recall steadily declined with lapse of time, while the *open-closed* steadily rose, until at the 130 days interval the number of items in which closure was effected almost doubled the number of items correctly recalled. It was found, too, that the advantage of the *open-closed* recall was relatively greatest in the case of Groups A and D, next greatest in the case of B and E, and least in the case of C and F, *i.e.* the groups with the small, medium, and large amounts of non-closure respectively. Three facts were thus apparent: (1) in all cases closure was effected, (2) in all instances, the greater the time-interval the greater was the closure, (3) the smaller the degree of non-closure on the charts studied, the greater was the closure effected on recall. Incidentally the results bore out the results in Wulf's experiment in which he found that figures when reproduced, tend either to be more regular than the original ones, or that characteristic traits are considerably exaggerated.[7] In Groups C and F, those figures which were persistently reproduced as open in the successive recalls, diverged more and more from the shape of the complete figure of which they were a part, though always retaining some feature which indicated the figure *S* had in mind. The fact that, even in this group, a larger and larger number of open figures were closed as

[7] F. Wulf, *op. cit.*, 373.

time went on, indicates that the majority of the open figures on Charts C and F were sufficiently suggestive of the symmetrical whole to draw the *Ss* in the direction of that whole rather than induce them to emphasize the abnormality.

In only a few scattered cases did the *Ss* in any group effect non-closure, *i.e.* reproduce a figure as open which was closed on the chart. The cases were too few to handle for statistical purposes. The tendency was for even such rare cases to drop out as time went on. There were practically none in the third and fourth recalls.

Age. In an attempt to investigate the effect of the *Ss*' age upon closure, the total group was divided into two sub-groups, a junior and a senior. The former consisted of 159 *Ss* 11-yr.-old and under; the latter of 156 *Ss* over 11 yr. of age. The same two factors were separately considered here which were dealt with in the case of the total group, viz., the comparative amounts of open and closed recall, and the question of closure.

With regard to the first question it was found that the senior group exceeded the junior in the total amount of open figures recalled at all time-intervals, and in terms of all degrees of non-closure on the recall charts, except in the case of the fourth recall (interval of 130 days), and here only in the case of the group that had studied the charts C and F. The older group also exceeded the younger in the total amount of closed figures in all instances. These findings were expected, since they are merely illustrations of the superiority in memory span which the senior group, other things being equal, should possess by reason of their superiority in chronological age. They have no crucial bearing on our question of closure. When, however, the relative position of the open and closed recall within each group was studied, the differences found, though slight, were significant. Comparing results in Groups A and D, B and E, and C and F, at all four recalls, the junior mean open recall almost invariably excelled their closed, while the opposite was true in the case of the seniors. It was evident that the non-closure of the figures on the charts influenced the recall of the junior group more than that of the senior.

As to the relative closure effected by the age-groups, a comparison revealed that the juniors excelled the seniors at all time-intervals and in terms of all degrees of non-closure on the recall charts. When open recall was expressed as percentage of total, it was apparent that the juniors had effected almost twice as much closure as the seniors under all circumstances. It was found that neither lapse of time nor degree of non-closure in the figures studied influenced the relative positions of the junior and senior groups with reference to either of the factors studied, viz., the comparative amounts of open and closed recall, nor the amount of closure effected.

Sex. Such sex differences as were found in the recall part of the experiment were slight, and were concerned more with the total amount of recall than with the relative position of open and closed figures within each group. The investigation proceeded along the same lines as in the case of age differences. The writer first looked for differences in the matter of relative recall, and then for differences in the effecting of closure. In neither of these matters did sex play a significant rôle.

Recognition Experiments

What is the Gestalt conception of recognition and of the difference between the latter and recall? Wheeler and Perkins say that recall is a partial perception.[8] It is an attempt to perceive over again the original object, under the handicap that important stimuli are missing. This notion is very different from the traditional one that recall is the reinstatement of a past impression, or the stimulation of traces left from former experiences. Köhler maintains the *non-process* theory of traces, which is essentially a Gestalt conception in that it entails a dynamic readjustment between two levels of equilibrium—a totally different thing from the older conception of the trace.

The recognition-process is supposed to take place in this way. When the stimulus-pattern is organized, simple perception takes place. Then something occurs which demands a reorganization of the stimulus pattern, *e.g.* a face in a crowd looks familiar. The question arises: Where did I see that face before? The face stands out from the present stimulus-pattern and does not harmonize with it. Tension arises. Then a new configuration has to be set up, viz., some former situation with the face as part of the stimulus-pattern. The process of recall then comes in, with its searching for, and selection of cues; the right cues bring back the former situation. The tension is relieved and the recognition-process stops. The face merges into the present stimulus-pattern. Recognition is thus not the result of the mechanization of processes, as in the old trace theory; it is a matter of organizing the brain activities into more complex and highly organized configurations.[9]

Woods found that a feeling of familiarity was the most outstanding feature of recognition.[10] The response is essentially motor in character, involving muscular sensations, and in making the response the organism acts essentially as a whole.

Hollingworth says that recall is that aspect of the memory process in which a setting, background, or association-cluster is present in clear consciousness, but a

[8] R. H. Wheeler and F. T. Perkins, *Principles of Mental Development*, 1932, 394.
[9] Wheeler, *The Science of Psychology*, 1929, 275.
[10] E. L. Woods, An experimental analysis of the process of recognizing, this Journal, 26, 1915, 313-327.

desired focal element is missing.[11] Recognition is the reverse of this process. In recognition the focal element is present, in the form of sensation, image, or feeling; the task is to supply the general setting or background. Both processes involve a common neural pattern operating in reverse directions in the two cases. The element of guessing must be taken into account as being much more potent in recognition than in recall. A learner can either recall a word or he cannot, but he may *say* he recognizes a word without either being sure that he does so, or without really recognizing it at all.

Method and Procedure

Materials. In addition to the 6 charts used in the recall test, another chart, called the recognition chart was prepared, depicting all of the figures used on all of the recall charts, *i.e.* showing each of the 12 geometric figures closed, and in each of the three stages of non-closure. This made 48 items in all. The figures were arranged on the chart in rows of 4, each row consisting of one figure in its several variations. Care was taken not to have the figure variations arranged in symmetrical order. This was to prevent the *Ss* from systematizing their responses rather than making a calculated selection of each individual figure. A separate recognition chart was available for each subject of the recognition test.

Subjects. The *Ss* for the recognition test were 329 pupils from St. Cecelia's school in the city of Omaha. They comprised all of the grades from the fourth to the eighth inclusive. Their chronological ages ranged from 7 to 14 yr. in terms of last birthday. Their mean E.Q. on the Stanford Achievement Test was 107; the range being 88 to 108. Their home environment was that of the middle class.

Procedure. As in the recall experiment, the *Ss* were divided into 6 sub-groups fairly equal in terms of age and sex, one group for each of the six charts: A, B, C, D, E, and F. There was one important variation from the technique employed in the recall test, viz., that while in the latter the same sub-group was used in all of the 4 recalls, every *S* making his recall 4 times, this was not done in the recognition test. To do so would introduce a complicating factor, viz., fresh stimuli in the form of successive presentations of the recognition chart to the same *S*. The effect of this would be difficult to calculate. Wheeler and Perkins found that successive reproductions of figures in a recall test similar to the first part of the present experiment, furnished stimuli which retarded the change toward symmetry which lapse of time was bringing about.[12] Successive presentations of the recognition chart to the same *S* in the present experiment would be likely to have an even greater inhibiting influence. It was decided therefore, to give every *S* only one recognition test. In these circumstances, to secure results in terms of time-intervals, every sub-group had to be again divided into 4 parts, one part for each of the time-intervals, viz., immediate recognition, and delayed recognitions of 11, 46, and 130 days. This made the individual groups somewhat small, especially as comparisons in terms of age and sex had afterwards to be made within each group.

As in the recall test, the work was carried out by the writer in the various class rooms during the afternoon sessions. The *Ss* were not given previous notice as to the immediate or delayed recognitions. The instructions were given in the manner stated

[11] H. L. Hollingworth, Characteristic differences between recall and recognition, this Journal, 24, 1913, 532-544.

[12] Wheeler and Perkins, *op. cit.*, 394.

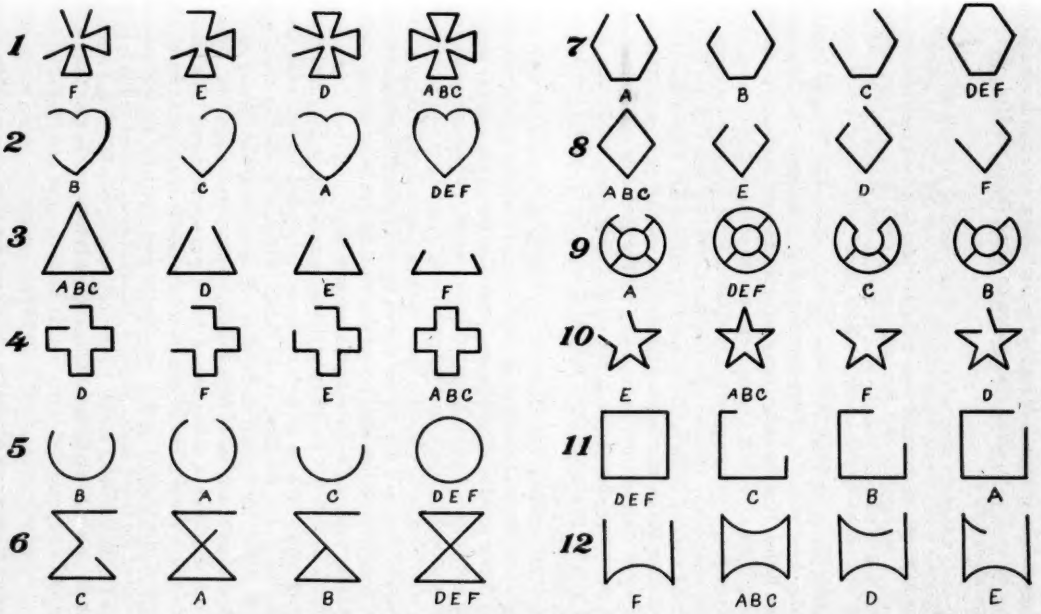

FIG. 2. THE RECOGNITION CHART

Letters under the figures indicate the Study Charts in which they appeared.

below. No *S* was allowed to ask any question, and care was taken to secure the keenest concentration on the part of the *Ss*. The recall charts were distributed as in the recall experiment. The *Ss* signed their names on blank sheets provided, then they were instructed as follows:

On the other side of the paper which is now on your desk you will see 12 figures. When I give the word, I want you to turn the paper over and study the figures. You will get only 40 sec. to do so, so keep looking at the figures all the time. Be sure to study *all* of the figures. I will then ask some of you to pick out from among a lot of other figures the ones that you have seen. No one is to speak during the test, and of course no one is to look at any one else's paper.

The blank papers on which the *Ss* had written their names, together with the identification letter of the chart they had studied, were collected. The study charts were also collected. Then the recognition charts (Fig. 2) were handed out to the 25% of the group seated in the back row for the immediate recognition—the second, third, and fourth quarters of the group were to be reserved for the delayed recognitions. The pupils were again briefly reminded what they had to do. They were told that they had seen one, and only one, of the figures in each row, and that they were to select on the recognition chart the one that they had seen and put a pencil mark on it. They were instructed not to omit any row, and that they were to mark only one figure in each row. The charts were then signed. At the delayed recognitions, the *Ss* were simply furnished with the recognition charts and told to select the figures that they had seen during the previous study.

Scoring. The following responses were possible in the recognition test: Every figure studied on the recall charts could (1) be correctly selected on the recognition chart, (2) closure could be effected in one or more degrees, (3) non-closure could be effected in one or more degrees. By closure being effected is meant choosing a figure on the recognition chart which would be more closed than the one seen on the recall chart; by non-closure being effected is meant selecting a figure on the recognition chart which would be more open than the one seen on the recall chart. The complete results, including degrees of closure and non-closure, were tabulated, but for the various comparisons the writer did not take degrees into account.

Weighting. The chances for effecting closure as against those for effecting non-closure were not the same in the cases of *Ss* who had studied different recall charts. Those who had studied the Charts A and D had 30 chances of effecting non-closure, and only 6 of effecting closure; that is, they had three chances of opening each of their 6 closed figures, and two of further opening each of their 6 open ones, whereas they had only one chance of closing each of their 6 open ones. Similarly those who had studied the Charts B and E had 24 chances for non-closure as against 12 for closure. In the case of the Charts C and F the chances were equal for closure and non-closure, each of the closed figures having 3 chances of being opened and each of the open ones 3 chances of being closed. To get correct results, therefore, it was necessary to weight the closure responses in the Charts A and D by 5, and in the Charts B and E by 2.

RESULTS

In general there was a decided tendency for the *Ss* to recognize the closed figures that they had seen on the charts better than the open ones. In the immediate recall the differences were considerable, but with lapse of time,

except in the case of the Charts B and E, the advantage ran now with one group, now with the other. Lapse of time in itself did not seem to have any consistent effect, neither did degree of non-closure on the charts studied.

Closure. It was found that the closure effected by the group unclassified in terms of age or sex exceeded the non-closure in almost all instances. There was practically no non-closure in the recall test, but in the recognition test a considerable amount of it appeared. This may have largely been due to the factor of guessing which, as Hollingworth states must be reckoned with in recognition, though not so much in recall.[18]

Age. A study of the correct responses in the recognition test within each of the two age groups disclosed: (1) in nearly all cases junior closed responses exceeded junior open; (2) in the senior group the open and closed correct responses were about equal. Neither lapse of time nor degree of non-closure on the charts studied had any significant effect on these relative positions.

With respect to the effecting of closure and non-closure it was found that the junior group had an average excess of closure over non-closure in the various recalls as follows, 6.00 being the maximum possible: Charts A and D, 1.17; B and E, 1.79; C and F, 0.77. The seniors had an average excess of closure over non-closure as follows: Charts A and D, 0.51; B and E, 0.98; and C and F, 0.01. The junior group, therefore, showed a greater tendency to effect closure in the recognition test than the senior group. This result is similar to that found in the recall part of the experiment. Lapse of time had no significant effect on the relative positions of the junior and senior groups as to the effecting of closure, neither did the degree of non-closure on the recall charts.

Sex. In all particulars the results as to sex were in agreement with those found in the recall test, that is to say, sex has no significance with regard to the closure phenomenon within the limits of the study.

Summary and Conclusions

The results of the recall experiments may be summarized as follows:

(1) There is a tendency, not marked enough to be statistically reliable, but fairly consistent nevertheless, for non-closed figures to be recalled more easily than closed ones. Neither time-interval nor degree of non-closure of the figure perceived has any measurable effect on this tendency.

(2) There is a marked tendency to close non-closed figures in recall.

[18] Hollingworth, *op. cit.*, 554.

The tendency is greatest in the case of the figures with the smallest degree of non-closure, and least in the case of figures with the greatest degree of non-closure. The tendency increases with lapse of time.

(3) The tendency to recall open rather than closed figures is greater in the case of *Ss* from 7 to 11 yr. than in the case of *Ss* from 11 to 14 yr.

(4) Closure is effected on recall to much greater extent by *Ss* 7- to 11-yr. old, than by *Ss* 11- to 14-yr. old.

(5) The factor of sex has no bearing either on the relative amount of recall of open and closed figures, or upon closure effected on recall.

(6) Non-closure is almost never effected in recall.

The recognition experiment yielded the following results:

(a) *Ss* 7- to 9-yr. old are more likely to correctly recognize a closed figure than an open one; *Ss* 12- to 14-yr. old seem to have equal facility in the recognition of open and closed figures.

(b) There is a much greater tendency to effect closure than to effect non-closure on recognition, that is to say: when errors are made in the recognition of forms, they are more likely to go in the direction of greater symmetry than in the direction of less, in terms of the forms actually perceived.

(c) The tendency for errors in recognition to go in the direction of closure rather than of non-closure is more apparent in *Ss* 7- to 11-yr. old than in *Ss* 11- to 14-yr. old.

(d) The factor of sex has no bearing on recognition within the limits of the study.

THE RÔLE OF INCENTIVE IN CONDITIONING AND EXTINCTION

By W. J. BROGDEN, E. A. LIPMAN, and ELMER CULLER, University of Illinois

In this paper we report four brief experiments upon the rôle of incentive in conditioning and extinction. These experiments demonstrate, in our opinion, the genetic identity of these two processes. In each case we have used the motor outlet, believing that skeletal musculature, because of its greater variety and flexibility of pattern, is a more suitable medium than salivation for investigating conditioned phenomena. The salivary techniques deal almost wholly with *magnitude* of response (number of drops secreted); rarely indeed with quality or kind. This procedure is analogous to measuring, say, the evolution of heat or output of CO_2 in a group of muscles with no regard for the pattern or character of the activity. The salivary mechanism is relatively inflexible, whereas the qualitative variations in the conditioned muscular response are limited, in number and variety, only by the experimenter's criteria. Even when gross muscular activity remains constant, the qualitative factors may reveal changes of utmost significance.

Experiment I. Here our purpose was to investigate the function of reënforcement in the conditioning process. It has been repeatedly emphasized by one of us[1] that the US plays a dual rôle; it determines the character or pattern of CR and it provides the energy or drive needed to actuate the response-pattern.[2] With the second of these functions we are now concerned.

Eight guinea pigs were prepared in the rotator,[3] a modified activity-cage which has been found useful in conditioning small animals. Four of the

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[1] E. Culler, Recent advances in some concepts of conditioning, *Psychol. Rev.* (in press).

[2] Abbreviations: US = Unconditioned (original) Stimulus; UR = Unconditioned (original) Response to US; CS = Conditioned (substitute) Stimulus; and CR = Conditioned (substitute) Response.

[3] W. J. Brogden and E. Culler, Device for motor conditioning of small animals, *Science*, 83, 1936, 269.

animals (comprising Group A) were trained by the methods commonly employed in this laboratory; that is, the US (shock) occurs after CS (1000-cycle tone) has continued just two seconds. The rotator is so devised that the animal, upon turning the cage an inch or more when the sound begins, escapes the shock by breaking the high-voltage circuit through a pendulum switch. Twenty-five trials were run each day, the ordinates being therefore percentages of that number. The four pigs required from 5 to 13 days (125, 175, 200, 325 trials) to reach a perfect score; the results were combined into a single score by the Vincent method. The animals pro-

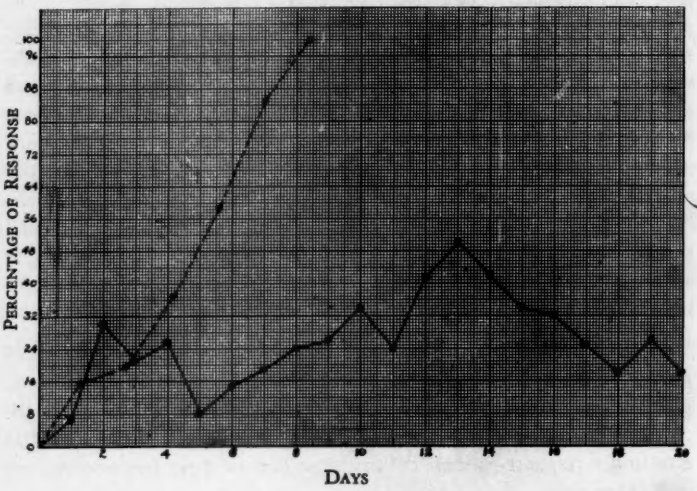


FIG. 1. CONDITIONING OF GUINEA PIGS WITH AND WITHOUT ESCAPE

----- Group A, with escape
——— Group B, without escape

gressed rapidly to a perfect performance (Fig. 1, dotted line). Those of Group B (continuous line) though exposed to the same situation (same sound, same shock, same test-environment) were not allowed to escape punishment by reacting; on the contrary, each stimulus-presentation was reënforced with shock, whether the animal turned the cage or not. As we see, the members of Group B began precisely as did the other four and gave every preliminary indication of advancing in the same way to full conditioning; but soon the incipient adaptation broke down. The pigs became erratic, rising or falling in random fashion from day to day; by the

end of 500 trials, the picture remained still the same. Nothing was to be gained by turning the cage since the shock came anyway; so when the tone began, they literally 'sat tight,' held the breath, and tensely awaited the shock.

Since the two experimental conditions differ only in frequency and distribution of incentive, it is clear that we can no longer generalize from the familiar Pavlovian situation, where the salivary response is always reenforced with food. *How* and *when* the incentive is applied determine the rate and character of resultant conditioning. Every stimulus asks a question or sets a task; and the CR which emerges constitutes the organism's reply to that question. One group finds a solution by turning the cage; the other, faced with unavoidable shock, so places and tenses the trunk and limbs as to minimize its effects. The functional significance of CR is crucial in deciding what form it shall assume; and this significance is itself altered when the incentive is differentially applied.

Experiment II. Pavlov reports that if the CS be continued without reenforcement after its CR has fully disappeared, the extinctive process will likewise continue (silent extinction); the evidence being that under such conditions the time requisite for spontaneous recovery is lengthened.[4] Our second experiment is designed to examine this "silent extinction."

Four dogs were conditioned to flex the right forelimb in reply to a 1000-cycle tone, whose amplitude was ca. 72 decibels above the animals' normal limens.[5] When they had reached the criterion of 100% CR (25/25 correct responses in one test-period), shock was omitted, whereupon CR subsided to zero. Two of the dogs were then at once reconditioned by again introducing shock. With the other two, extinction was continued, tone alone being used, for 400 extra trials; whereupon they too were reconditioned with pedal shock up to 100%. All four being then at the same level, two having been just barely extinguished (A), the other two having been exposed, before re-conditioning, to 400 cases over and beyond bare extinction (B), the procedure was reversed for each group. The presence and magnitude of silent extinction could thus be measured by the *amount of incentive* (number of shocks) required to recreate CR.

The data can be quickly epitomized under two heads. *(1)* More shocks were needed in each case for reconditioning after B than after A: 11 more for dog 1, 7 more for dog 2, 11 more for dog 3, 5 more for dog 4, a mean excess of 8.5. In other words, 13.75 shocks were required to overcome

[4] I. P. Pavlov, *Conditioned Reflexes* (G. V. Anrep, tr.), Oxford, 1928, 57.
[5] E. Culler, G. Finch, E. Girden and W. J. Brogden, Measurement of acuity by the conditioned-response technique, *J. Gen. Psychol.*, 12, 1935, 223.

the effects of B, whereas 5.25 shocks served for complete reconditioning after A. These figures may be accepted as showing what usually happens in the *first* stages of extinction and reconditioning. Later, when the dog has often undulated from 100% to 0% to 100%, all trace of silent extinction vanishes. Of the dogs used in Experiment IV below, one (Sibling), after being once just barely extinguished to zero, took 10 shocks to recondition; the second time, after receiving 1250 added extinction-trials beyond zero, only 8 shocks. Another (Sherrington) was extinguished no less than 7 times. Reconditioning required, after the first extinction, 18 shocks; after the second, 11 shocks; after the third, with 450 cases of added extinction beyond zero, 5 shocks; after the fifth, with 450 cases of added extinction, 1 shock; after the seventh, with 850 cases of added extinction, 2 shocks. One shock being the utter minimum for any reconditioning, we see no trace of silent extinction in the last two of these. The phenomenon proves therefore to be restricted to the earlier cases; later, after several cycles up and down have been traversed, the animal changes from negative to positive like snapping a switch, whatever degree of extinction may have anteceded the moment of change.

(2) The conditioning-curve was far steeper than the extinction-curve. The figures for dogs 1, 2, 3, 4, follow: the second conditioning occupied 125, 100, 75, 75 trials, a mean of 94; the second extinction directly thereafter consumed 200, 350, 175, 475 cases, a mean of 300; the third conditioning in turn took 100, 75, 175, 125 trials, a mean of 119. Extinguishing CR takes three times as long as upbuilding it. Why is this? Whereas conditioning is activated by a potent excitor (shock), the only incentive for extinction is inertia. The dog might keep on reacting forever were it not easier now and then to stand still. Let us enforce the extinctive process more effectively and see what happens.

Experiment III. Two dogs were conditioned exactly as in Experiment II, save that a leather belt with two electrodes was fastened about the thorax. As soon as CR reached 100% the pedal shock was omitted; the same voltage being now applied (not to the paw but) to the left side of the chest. Instead of being shocked on the foot when he *failed* to react, the dog was now shocked on the ribs if he *did* react to the tone. If the right foreleg was flexed at all during the 2-sec. sound, the charge was automatically applied. After having been duly accustomed to both incentives, the dogs were first extinguished *with* thoracic shock (A); then reconditioned with foot-shock; then again extinguished *without* thoracic shock (B), tone being given alone. The full series is ABBA.

The observations justify two statements. (1) Thoracic shock greatly accelerates the course of extinction. In Dog 5, Method B required 337.5 trials, Method A 75.0 trials (mean of two cases each). In Dog 6, Method A first takes 150 trials; then Method B is run for 1000 trials, whereupon CR remains still at 80%. To save time, extinction was then completed by Method A in 175 trials. (2) Whereas in Experiment II extinction took

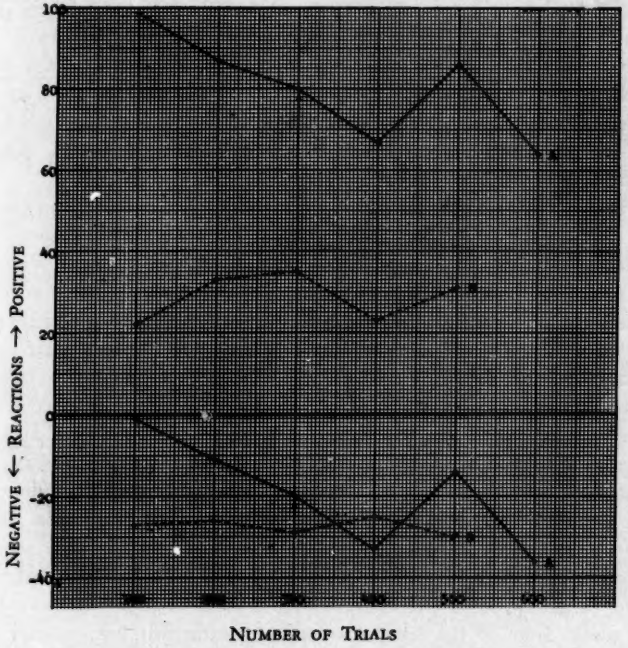


FIG. 2. APPARENT 'EXTINCTION' IN THE DOG
Positive and negative paw-movements in the conditioned animal (A) and in the unconditioned animal (B) with a 1000-cycle tone.

three times as long (300 trials vs. 94 and 119) as reconditioning, extinction now occupies about the same time or less. In Dog 5, reconditioning (mean of 4 cases) requires 181 trials vs. 75 for A-type extinction. In Dog 6, reconditioning (mean of 2 cases) needs 125 trials while the one case of A-extinction used 150 trials.

From these observations (Experiments II and III) we see that extinction, when duly energized, operates just as conditioning does. During thoracic

extinction it was observed that the dog would bear down during the bell; instead of withdrawing the paw, he would press it firmly against the grid. This fact suggested that extinction of a positive CR (flexion) consists in establishing a negative CR (extension); and led to the next experiment.

Experiment IV. Five test dogs were conditioned as usual to a tone; the shock being now applied via two electrodes attached to the right forepaw, which rested on a large rubber tambour instead of the metal grid previously used. The tambour was connected with a polygraph outside the test-room. By this closed pneumatic system every movement of the forepaw, whether up or down, as well as onset and end of both stimuli, were all projected onto the moving stylograph-tape. In this way ordinary observation of the animal's gross behavior was supplemented with a minute record of sub-visible motions and postural changes.

Fig. 2 gives a graphic sample of the results, which may help to clarify and confirm the hypotheses above framed. Each work-period comprised 25 trials, but the results are plotted in units of 100 for economy of space. All records with no detectible excursion of the tambour are listed as zero; all upward movements of the paw, however slight, are listed as positive and plotted above zero-line; all downward pressures, however feeble, are listed as negative and set below zero. The two unbroken lines (A) trace a representative extinction-curve of one test-dog (Emmy). From complete conditioning (100% up, 0% down) the positive scores decline and the negative enlarge. The vertical arrows (↑) mark the locus of conventional extinction; that is, the period when first the dog stopped withdrawing paw from grid. Despite this virtual cessation of gross leg-flexion, slight shifts up and down still persisted and registered on the stylographic record; apparent extinction thus proves to be far indeed from complete inaction. After the arrow, positive reflexes continue strongly to preponderate; indeed the results from all the dogs, *after* apparent extinction, are: positive displacements, 49.2% (of the whole number), negative 21.9%, and zero 28.9%.

The broken lines (B) of Fig. 2 display a fair sample of activity in the controls. These animals were wholly untrained, never having been in the test-stock before. They were run day by day, from 16 to 38 periods, to discover how a naïve animal behaves when exposed time after time to sound alone. In (B) every paw-reflex during the tone-period was tabulated for 500 trials. Though exhibiting the same play of opposing impulses, up and down, as characterize the trained animals (A), the total picture is quite unlike. Here positive and negative are virtual mirror-images of each

other; up and down displacements occur with equal overall frequency. For the five controls, the up-scores equal 31.4% of the whole, down scores 30.7%, zero 37.9%.

Since 30% of the control reflexes are negative, it might appear that the down-reflex during extinction is but the animal's normal response to tone alone and has no relevance here. These figures illuminate the difference. During the first four periods (100 cases each), the five controls registered 152, 139, 130, 130 pressures; for the five test-dogs the pressures run 25, 38, 60, 114. The control animals pushed down at the first more often than later; while the dogs under extinction pressed down little at first, then more and more.

These results demonstrate how much an animal which, by conventional criteria, is 'extinguished' may differ from one unconditioned; and they also show that the apparent repose of a naïve dog when exposed to tone alone is not a state of inaction but rather a labile balance between opposing forces. The dog when 'at rest' is oscillating about zero, positive impulses alternating with negative.

Interpretation and Conclusions

(1) In Experiment I, the same CS (tone) and the same US (electric charge) produce when combined two contrary forms of CR. One is a fair replica of UR (springing forward and spinning cage), the other consists in tensing the body-musculature and waiting motionless for the shock. These CR cannot both be described as the "same response to a substitute stimulus;" each is rather a response in its own right, its function being in our view to serve as 'primer' for the oncoming US. In some cases, the optimal way to prime for a given stimulus is to 'warm up' the usual reaction to that stimulus, like the pitcher who throws a few balls before facing the batter; but, in other cases, the preparation may be very different indeed. The best way to get ready for an oncoming car does not consist in falling violently to earth and writhing about, as one might do if actually struck down by it. Since the pattern of UR is identical for both groups of guinea pigs here used, the divergence of these CR may properly be ascribed to incentive alone. There is nothing mysterious in this differentiation of two CR once their character is dynamically conceived.

(2) In Experiments II and IV we have some evidence on 'silent' extinction in dogs. Over twice as much incentive (shocks) was needed to rehabilitate CR when the extinction-procedure was carried 400 trials beyond zero as when this procedure was stopped at zero. While this was true in the first two cycles of extinction and conditioning, in later stages all trace

of silent extinction vanished. Even though the animal be run, after apparent (gross) extinction, for many-hundred trials over a period of weeks without a single shock, it will switch back to the level of 100% CR as soon as one or two shocks are again applied. The concept of silent extinction is therefore narrowly qualified in both range and utility. Even where the phenomenon appears, it indicates but half the story. Extinction and conditioning are two sides of the same picture, reverse and obverse. In the 'natural' animal, positive and negative impulses oscillate about zero; failure to respond to the bell reflecting the perfect balance with which the impulses interplay (31.4% plus, 30.7% minus). Under conditioning, the positive reflexes are strongly fostered and the negative correspondingly inhibited; while in extinction the positive are depressed and the negative promoted.

(3) On the conclusion that extinction is not mere absence of conditioning but rather a mechanism in its own right, Experiment III tests the effect of accentuating this mechanism. By use of a shock applied to the left thorax of the dog, extinction can be so much accelerated beyond its normal pace as to equal or actually to exceed the speed of reconditioning.

(4) Experiment IV seeks to expose the more intimate nature of this extinctive process. The animals used are conditioned and extinguished in the usual way (without thoracic shock); but the active paw now rests upon a large rubber tambour, so connected with a polygraph that every up or down movement, however slight, registers on the moving tape.

(a) Even when the dog has reached apparent extinction (no gross foot-withdrawals), it is in every case far indeed from the status of an untrained animal. After 300-1250 unmotivated (tone alone) trials following apparent extinction, positive reflexes are still more than twice as numerous as negative (49.2% vs. 21.9%); whereas in dogs prior to all training positive and negative impulses exactly balance (31.4% vs. 30.7%).

(b) In every case, the flexion-reflexes decline under ordinary extinction during the first 300-600 trials; thereafter they stabilize near 50% (as vs. some 22% pressure-reactions) in the mean for all dogs.

(c) Prolonged extinction with no incentive (save what is provided by sheer inertia of movement) shows no sign of reaching the equilibrium which existed prior to conditioning. In a word, *unmotivated extinction remains incomplete,* even as does unmotivated conditioning. After 1650 consecutive extinction-trials in one dog, the *plus* reflexes remained at 42.4%, *minus* at 26.5%, the remainder being *zero.* We see how persistent is the imbalance introduced by conditioning, and how vital is the rôle of incentive in restoring the animal to its normal equipoise about zero.

(d) That extinction is a form of negative or inverse conditioning has

been formerly suggested by one of us (Culler);[6] but we did not then know—what these data from the motor channel reveal—that the normal state is one of balance, which conditioning temporarily overrides.[7] The symmetrical distribution of reflexes about zero (31.4% *plus*, 37.9% *zero*, 30.7% *minus*) is, by means of neural energy generated through charge-to-paw, so sharply skewed upward that it becomes under complete conditioning 100% *plus*, 0% *zero*, 0% *minus;* under extinction, the incentive provided by inertia rebuilds the negative and weakens the positive reflexes until they stabilize near the mean values; 49.2% *plus*, 28.9% *zero*, 21.9% *minus*. Only by use of stronger incentive (thoracic shock) can the animal be restored to its normal state prior to conditioning.

[6] Culler, *op. cit.*

[7] G. R. Wendt, An interpretation of inhibition of conditioned reflexes as competition between reaction systems, *Psychol. Rev.*, 43, 1936, 258. Wendt has recently rejected extinctive inhibition upon somewhat similar grounds and has suggested instead the more general principle of competition between behavior-systems. An activity is inhibited when another reaction-system takes its place.

THE EFFECT OF PAIN UPON PERIPHERAL BLOOD VOLUME

By JOHN PAUL NAFE and KENNETH S. WAGONER, Washington University

Many studies have been made on the effect of stimuli upon circulatory changes and many authors have considered the possibility of a relationship between vascular activity and sensory experience. Divergent results obtained by many experimenters have tended to render the conclusions doubtful and consequently no specific relationships have been definitely established. Among the methods applied to the study of vascular changes, the use of the plethysmograph has been none too satisfactory in yielding similar results for different experimenters. In the work on pleasantness and unpleasantness most of the experiments show, however, that unpleasantness is accompanied by vascular constriction.[1] Apparently little work has been done on the effect of pain-stimulation upon dilation or constriction of the peripheral blood vessels.

In view of the theory which proposes that certain types of pain may result from patterns of nerve impulses set up by constriction of the blood vessels, pain-stimulation might be expected to cause vascular constriction. If spastic contraction of the muscles of the blood vessels occurs in a localized region of the skin as a result of painful stimulation it is likely that such a constriction may be reflected over a larger area due to reflex activity. If such constriction takes place and is sufficiently wide spread it may result in an increase in arterial blood pressure. In a previous experiment it was demonstrated that certain types of pain-stimulation produced a rise in arterial blood pressure and it is possible that such a rise in pressure may be a result of vasoconstriction.

It is the purpose of the present experiment to determine the effect of painful stimulation upon certain phases of vascular action.

METHOD AND PROCEDURE

Apparatus. The apparatus used to record the blood volume changes was a modified form of the standard metal plethysmograph. All the recording apparatus rested on a platform suspended from an overhead frame. For a detailed description see Nafe and Wagoner.[2] A Marey type recording tambour was connected to an outlet

* Accepted for publication June 28, 1937.

[1] S. D. Robbins, A plethysmographic study of shock and stammering, *Amer. J. Physiol.*, 48, 1919, 285-330.

[2] J. P. Nafe, and K. S. Wagoner, The effect of thermal stimulation upon dilation and constriction of the blood vessels of the skin of a contralateral hand, *J. Psychol.*, 2, 1936, 461-477.

tube from the plethysmograph and the writing lever of the tambour recorded on a moving kymograph drum. A signal-marker showing the time of stimulation and a time-marker showing the speed of the drum also recorded simultaneously on the smoked surface of the drum.

Electrical stimulation was administered by means of a Harvard inductorium with the regular interrupter replaced by a 60 ~ oscillator. An attenuator was placed in the secondary circuit to vary the intensity of the delivered current (see Fig. 1). The stimulating electrodes were of the small platinum wire type with the points at the end spaced approximately 1 mm. apart. The duration of the electrical stimulation was controlled by a sliding contact made through small exposed metal segments of a slowly revolving kymograph drum. Such a contact closed a circuit which operated a relay which in turn closed the primary circuit of the inductorium by a mercury contact switch. This apparatus permitted stimulations of equal duration to be made from time to time. The attenuator permitted the current delivered to be regulated within a wide range, the units of intensity on the dial ranged from 1 to 10 and corresponded to a current range from 0.0001 to 0.20 milliamperes average current. (The attenuator dial units bear practically a logarithmic relation to the amount of current delivered, so that equal increases in terms of dial units results in much greater increments in average current.)

Heat-stimulation was administered by immersing the *O*'s hand in a container of water of the desired temperature. A lever arrangement was constructed for the

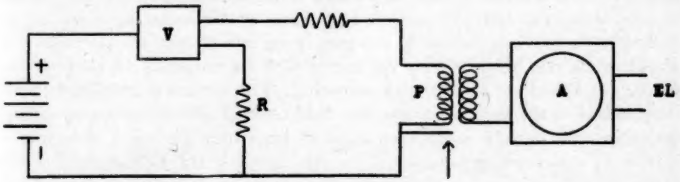


FIG. 1. DIAGRAM OF ELECTRICAL STIMULATOR
A=attenuator; EL=electrode leads; P=primary; R=5 ohms; V=60 cycle vibrator.

purpose of raising the container of water up over the *O*'s hand so that he was not required to make any movements during any part of the experiment. The water temperatures were measured by 1/10° mercury thermometers.

The skin was stimulated by the application of sharp needles by means of an instrument designed to control the intensity of the stimulus to some extent by the height from which the point of the needle fell upon the skin. The needle was attached to a vertically sliding holder which could be raised to varying distances above the skin and then released allowing the needle to fall directly upon the skin. An attachment permitted the insertion of 4 needles into the sliding holder at the corners of a 4-mm. square. The force with which the needle or needles fell could also be varied by increasing or decreasing the weight of the holder to which the needles were attached.

Experimental procedure. Five *O*s were used in this experiment. They were all men between the ages of 20 and 30 yr. and in good physical health. None of the *O*s had any understanding as to the essential nature of the problem. *O* was seated with the

right arm resting on the suspended platform with the right hand in the plethysmograph. The left arm and hand rested in a similar position on a small table at about the same height as the plethysmograph on the opposite side. The left hand was stimulated in this position with electrical and needle prick stimulation. The plethysmograph, provided with a thermometer, was filled with water at approximately 33° C. and a balance in pressure effected between the plethysmograph and the recording tambour. *O* was usually in the apparatus about 10 min. before any stimulation was administered. The stimulations were essentially of three types: electrical (faradic); heat (hot water); and needle prick. Most of the stimuli were applied to the back of the left hand and some to the back of the left forearm. The electrical stimulus could be applied at any intensity from a barely perceptible stimulation to an extremely painful one. In order to determine whether or not electrical or electrical-pain stimulation produced any vascular constriction in the contralateral hand the electrodes were simply applied (by the experimenter) to the area of skin selected at a given intensity of stimulation. The stimulus was applied to the skin for approximately 1 sec. and sometimes consecutive stimulations (2–5) were administered with an interval of approximately 1 sec. between the consecutive stimulations. The intensities used varied from non-painful ones to definitely painful intensities. *O* was instructed to report on the sensation accompanying the stimulation. The intensity of the delivered shock was recorded on the kymograph drum in terms of the attenuator dial units. In order to investigate any possible relationship between the intensity of the stimulation and the amount of constriction occurring, the duration of stimulation was carefully controlled by means of the automatic stimulator described in the previous section. In this part of the investigation the electrodes were placed on the skin before stimulation, resting with the weight of the electrodes and holder (11.4 gm.) on the area to be stimulated. With varying intensities of current, the duration of the stimulation was thus held constant and the effects of different intensities upon vascular constriction could be determined. The effect of varying the number of consecutive stimulations of the same intensity and duration could also be investigated. In order to provide a check upon the results, stimulations of sub-painful intensities were given as well as painful ones.

Hot water stimulation was applied to *O*'s left hand which hung down in a normal position and sometimes the left arm was forced slightly away from the body by pads in order to facilitate stimulation. During the experiment all movement on the part of *O* was as nearly as possible eliminated and for this reason stimulation with water was accomplished by raising the container of water over the *O*'s hand. The temperature of the water used depended upon the temperatures reported by the *O*s as painful. The approximate temperature was usually determined several minutes before the experimental tests were made. The use of sub-painful temperatures as a control was not essential to this part of the experiment since the effect of such stimulation had already been clearly shown.[3] Likewise, stimulation by application of hot air was not used in this experiment since the effects of such stimulation on contralateral blood volume have already been demonstrated.[4]

Pain aroused by pricking the skin with a needle was accomplished in some cases simply by the sudden application of a sharp needle to the skin of the back of the

[3] Nafe and Wagoner, *op. cit.*, 465-471.
[4] *Ibid.*, 465-471.

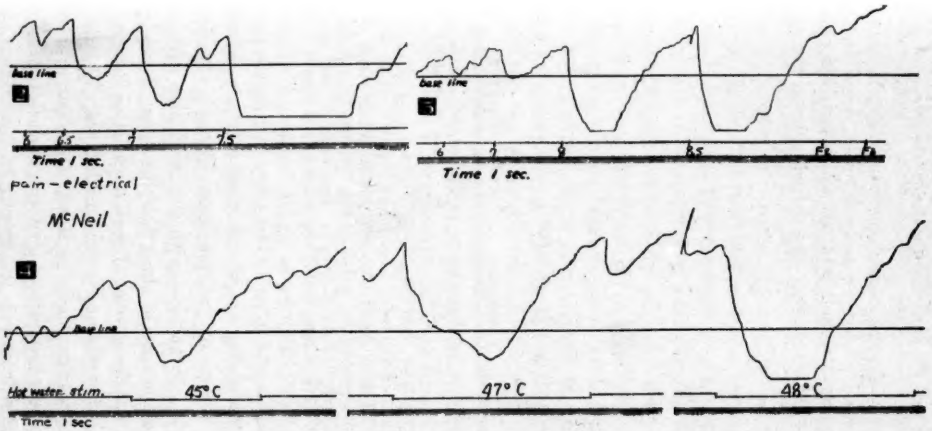


FIG. 2. ELECTRICAL PAIN-STIMULATION

The effect of electrical pain-stimulation is shown by the drop in the curve indicating constriction following each stimulation of 1-sec. duration. The intensity is indicated on the record. The last stimulation on right shows the writing lever down against tambour and consequently all the constriction is not shown.

FIG. 3. EFFECT OF INTENSITY OF ELECTRICAL PAIN-STIMULATION

The record shows the effect of four different intensities upon the amount of constriction. F_3 indicates stimulation by touching electrodes to skin without current. See also Fig. 2 which shows similar relationships between intensity of stimulus and amount of constriction. Similar intensities on different records are not comparable since different subjects do not yield the same amount of constriction with a given intensity of stimulation and frequently the sensitivity of the apparatus is changed at the beginning of a series of stimulations.

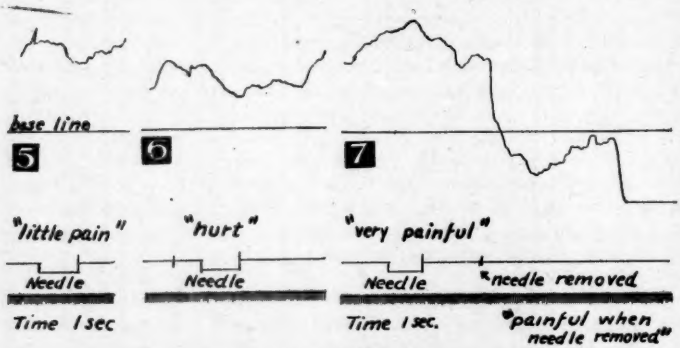
FIG. 4. EFFECT OF HEAT-STIMULATION (HOT WATER)

The effect of intensity of the pain-stimulus upon the amount of constriction is evident from the record.

hand with enough force to arouse pain. Other stimulations were given by the device above described. In many cases the height from which the needle fell or the force with which it was applied did not bear a close relationship to the intensity of the pain aroused and for the purpose of such comparisons the subject was instructed to state, upon a basis of comparison with the preceding stimulation, the amount of pain aroused.

RESULTS

Electrical stimulation. The results include 90 trials which involved pain, 16 stimulations in which the electrodes were applied without current and



FIGS. 5-7. EFFECT OF NEEDLE STIMULATION

Fig. 5. shows amount of constriction following pricking of the skin which was reported by *O* as "little pain." Fig. 6 shows same type of stimulation and *O* reported "hurt." Somewhat more constriction is shown. Fig. 7 shows the relative amount of constriction when *O* reported stimulation as "very painful," and also "painful when needle removed." The accompanying constriction is evident.

15 definitely sub-painful electrical stimulations. In every case where pain was reported with electrical stimulation the records show a clearly defined and definite decrease in the volume of the contralateral hand, indicating vascular constriction (Fig. 2). Stimulation with the electrodes without current sometimes showed no effect or, in about half the cases, showed a small amount of constriction but the amount of such constriction was consistently much less than that accompanying even the mildest pain. Electrical stimulation at sub-painful intensities produced a small amount of constriction but never as much as definitely painful ones, but as the sub-painful stimulations approached painful intensities the amount of constriction increased. Series of painful electrical stimulations, in which the duration was held constant and the intensity was varied, show a definite relationship between (1) the intensity of stimulation; (2) the intensity of the pain felt;

and (3) the amount of vascular constriction. For example (Fig. 3) stimulation with an intensity of 7 units upon the dial of the attenuator (0.076 milliamperes) produced more constriction than stimulation with 6 units (0.04 milliamperes) while 8 units (0.12 milliamperes) produced a greater amount than 7 units and 8.5 units (0.14 milliamperes) produced more constriction than an intensity of 8 units. The records show some inversions and in some cases the same intensity of stimulation produced somewhat different amounts of constriction, due probably to uncontrolled variables, but in most cases the relation between the intensity of stimulation, the degree or amount of vascular constriction and the felt intensity of the experience is clearly shown by the records and reports.

Heat stimulation. Pain produced by stimulation with hot water gave results similar to those obtained with electrical stimulation. It has previously been shown that temperatures of water above some 45° C. produce constriction of the blood vessels and that there appears to be a relationship between the temperature of the water and the amount of constriction.[5] The results of this experiment confirm the previous findings. It also has been previously shown that stimulation with temperatures less than those felt as hot, but above skin temperature, cause vascular dilation and are felt as warm. The records obtained in the present experiment show that all stimulations with water at a temperature sufficient to produce pain in a given *O* were accompanied by definite constriction of the blood vessels of the contralateral hand. For *McN* (Fig. 4) water at 45° C. was sufficiently hot to produce very slight pain and the accompanying constriction upon stimulation appears in the record. Stimulation with water at 47° C. was reported by this *O* as more painful and the corresponding amount of constriction is greater. Likewise, stimulation at 48° C. produces more constriction than at 47°. In this connection the wide individual differences between *O*s should be noted. Some *O*s report temperatures as low as 43.5° C. as very hot and perhaps mildly painful while other *O*s may not report the slightest pain or heat until the temperature of about 47° or 47.5° C. is reached. When the lower temperatures are reported by a particular *O* as hot or painful, the records also show that stimulation with this temperature produces constriction in that *O* while temperatures as high as 46° or 47° C. reported by other *O*s as only warm produce dilation of the blood vessels in those *O*s. These facts definitely show the relationship between the action of the blood vessels and the felt experience. It also should be noted that constriction may accompany stimulation reported as hot but not as painful but in these records whenever constriction is present *O* invariably

[5] Nafe and Wagoner, *op. cit.*, 465-474.

reports the water as "hot." As is to be expected there are wide differences in what individuals call "pain" and similar differences undoubtedly exist at the margin between "warmth" and "hot." The records, however, show that after a stimulating temperature sufficient to arouse mild pain has been reached, stimulation with higher temperatures consistently results in correspondingly greater constriction within the limits of the integrity of the tissue. The records show, again, as in electrical stimulation, some inversions and sometimes equal amounts of constriction with slightly different temperatures of water, but as a whole they are quite consistent in showing a definite relationship between the intensity of the stimulus, the action of the blood vessels, and the intensity of the felt experience.

Needle stimulation. Pain aroused by pricking the skin of one hand with a needle or needles was always accompanied by constriction of the blood vessels in the contralateral hand. *O* reported, after every stimulation, the relative intensity of the felt pain. The fact that the same intensity or force of stimulation with a needle on different skin areas does not always, or even often, arouse equal intensities of pain permits further study and support of the relationship between the intensity of the pain and the constrictive process since in cases in which only very mild pain is aroused such stimulation is accompanied by only a relatively small amount of constriction while a much greater amount of constriction accompanies tests in which exactly the same intensity of stimulation arouses greater pain. Some stimulations resulted in a rather long after-effect in which the pain continued for several seconds after the stimulus was removed and in such cases the constriction was invariably prolonged (Figs. 5, 6, and 7), while in those cases in which the pain disappeared rather rapidly after stimulation, there was usually a rapid recovery from constriction. As a control, extremely mild stimulation with a needle which did not produce pain was used. Small amounts of constriction sometimes accompanied such stimulation but the amount was never as much or as pronounced as that accompanying pain.

Discussion

The relation between the intensity of the stimulation, the amount of vascular constriction, and the intensity of the pain is definitely shown by the records and reports. It is true that in some cases of stimulation this relationship was not evident or an actual inverse relation obtained. Such instances, however, are the exception and the preponderance of positive cases is such as to clearly show an existing correspondence. Without doubt there are many uncontrolled variables functioning from time to time which

are either impractical to control or do not permit of it. The frequency of inversions will be dependent to some extent, as in other psychological measurements, upon the size of the interval. Considering such factors it would be improbable that every single stimulation should result in a perfect correlation and it is only by a considerable number of such stimulations that the true relationship obtaining between the essential variables may be shown.

With hot water stimulation it has been noted that with lower temperatures of hot water sufficient to produce constriction that not infrequently some *O*s may report such temperatures of water as hot but not painful. There is no discrepancy in the correlation of both 'hot' and 'painful' sensations with vascular constriction. Many *O*s report sensations of very mild pain accompanying stimulation with water barely hot enough to produce constriction. The fact that other *O*s do not report painful sensations with such temperatures may be due to a different criterion of pain assumed by these *O*s. It is not at all necessary, however, from the standpoint of the theory to assume the presence of any painful element in the quality of heat, since increasing the stimulating 'hot' temperature always results in pain sensation and a corresponding increase in vascular constriction. The important facts to be noted are: (1) that 'hot' stimulation invariably results in some amount of constriction which is, in some measure, proportional to the stimulating temperature; and (2) that a further increase in the intensity of the hot stimulation always results in definite pain with a corresponding increase in vascular constriction. Physiologically, then, the difference between 'hot' and 'painfully hot' becomes a matter of the degree of vascular constriction and the corresponding patterns of nerve impulses set up by such constriction.

No pain-stimulation by hot air was administered in this experiment since the results of such stimulation have already been demonstrated.[6] In that experiment it was shown that hot stimulations were consistently accompanied by constriction of the blood vessels of the contralateral hand, and presumably of both hands. At that time no attempt was made to determine exactly the relationship of the intensity of the pain aroused and the amount of vascular constriction, but in those cases in which only very mild pain was reported the amount of constriction accompanying such stimulation was consistently less than the amount accompanying stimulation with higher temperatures which produced more intense pain. There was no necessity for any control stimuli in the part of this experiment in which hot water

[6] Nafe and Wagoner, *op. cit.*, 465-474.

was used as a stimulus since it has already been shown that temperatures of water immediately below those reported as either hot or painful produce dilation of the blood vessels and not constriction.

The results of this experiment are consistent with those of a previous experiment in which pain was found to increase blood pressure. Presumably one factor operating to raise the blood pressure upon pain-stimulation is the more or less generalized vascular constriction accompanying pain-stimulation.

Conclusions

(1) Pain aroused by faradic stimulation of the skin of one hand is consistently accompanied by constriction of the blood vessels of the contralateral member.

(2) The intensity of the felt pain bears a definite relationship to the amount of vascular constriction.

(3) Pain aroused by stimulation of the skin with hot water is consistently accompanied by constriction of the blood vessels of the skin.

(4) The intensity of the felt pain aroused by hot water, the intensity of stimulation (temperature), and the amount of vascular constriction are definitely related.

(5) Pain aroused by needle prick is accompanied by constriction of the blood vessels of the skin.

(6) The intensity of the pain aroused by needle prick and the degree of vascular constriction are related.

(7) Electrical and needle prick stimulation of the skin with subpainful intensities sometimes causes a slight amount of constriction but when such constriction occurs the amount or degree is not as great as that accompanying pain from these sources.

(8) Stimulation of the skin with sub-painful intensities (temperatures) of hot water is accompanied by a relatively small amount of constriction as compared to painful stimulation while still lower temperatures produce dilation of the blood vessels and accompanying warmth sensations.

(9) The results of this experiment indicate a definite relationship between the sensation of pain as elicited by these types of stimulation and the constrictive action of the muscles of the blood vessels of the skin which accompanies such stimulation.

THE DISCRIMINATION OF COLOR MASS

By B. R. PHILIP, University of Alberta

Most tests of color perception yield a qualitative rating only, and classify the subjects in a discrete manner. On the assumption that ability to perceive color is distributed in the general population as a continuum the test here described attempts to assign a numerical rating, expressed as a percentile, to the color perceptive ability of each *S*. The ability tested is not that of the discrimination of the individual components of color, *i.e.* hue, saturation, and brightness, but rather the discrimination of what may be called color mass. Areas to be compared are distributed into elements of equal area so separated that their colors affect each other in a complex manner. *S* selects the color having the most elements, that is, the greatest area or mass.

METHOD AND PROCEDURE

Test material. The test material is contained in a pack of 40 cards, consisting of 20 different pairs of cards. On every card are printed the 4 colors, blue, green, purple, and red, in 36 dots, ¼ in. in diam., spaced ⅛ in. apart and randomly distributed according to the following scheme. One pair has 18 blue dots and 6 each of the other 4 colors. One pair has 16 blue dots and of the remaining colors, 7, 7, and 6 dots respectively. One pair has 14 blue dots, and 7, 7, and 8 respectively of the others. One pair has 12 blues and 8 of every one of the others. One pair has 11 blues and 8, 8, and 9 of the others. This scheme is followed for all 4 colors.

The box in which the material is contained has 4 compartments each indicated by one of the 4 colors. The task is to place the cards in the appropriate compartments according to the predominating color. The colors chosen were all of saturation 10 and brightness 5 on the Munsell Scale, and were matched as closely as possible with the red, green, blue, and purple-blue of that scale. An attempt was made to equalize the colors as closely as possible in saturation and brightness by the appropriate selection of especially prepared inks. When the colors as printed were individually compared for brightness on the color wheel a good but not perfect agreement was obtained. When the cards were constructed further divergencies from equal brightness and saturation were noticed due to factors to be discussed later.

Instructions. The pack is well shuffled; *S* is placed at his ease with the material in front of him so that he may see the cards by excellent daylight. The following instructions are read to every *S*.

On each card of this pack are dots in 4 colors, blue, green, purple and red. On every card there are more dots of one particular color. The cards are to be sorted into the four compartments of the box according to the predominating color; the ones with most red dots on them in the red compartment; the ones with the most green dots, in the green compartment, etc.

* Accepted for publication December 12, 1936.

Do not try to count the dots, but judge which color occurs in the greatest amount. Accuracy and speed both count; go as fast as you can without making mistakes. If in doubt judge as well as you can. Do not pick up the pack till I say 'Start!' When you have finished say 'Ready!' and leave the cards in four piles. You have three trials.

Scoring. Three and one half minutes are allowed per trial. Cards then unsorted are counted as errors in addition to those wrongly sorted. Only a small percentage of *Ss* fail to finish on time. The time for each trial is counted in seconds as estimated on a stop-watch and is recorded on a score sheet. The errors are tabulated as follows. On the reverse side of each card is marked the initial of the correct color together with the number of dots of that color on it. *E* goes through the cards that have been sorted in the compartments, selects the cards incorrectly placed in that compartment and tabulates them in the appropriate place on the score sheet. When the errors have been tabulated the cards are re-shuffled and replaced for *S* to start a new trial.

Subjects. The *Ss* were all boys. Their ages and grades in school are listed in

TABLE I

MEAN SCORES ACCORDING TO AGE

Ages*	10 and 11	12	13	14	15	16	17	18 and 19	Total
No. *Ss*	36	77	73	81	76	73	47	22	485
Means	28.0	29.5	28.8	30.0	29.0	27.8	30.3	27.6	29.05
σM	2.15	1.58	1.76	1.71	1.45	1.38	1.83	1.68	.605

* Age 12 means from 11 yr., 6 mo. to 12 yr., 6 mo., etc.

TABLE II

MEAN SCORES ACCORDING TO SCHOOL GRADE

Grades	5	6	7	8	High school	Ungraded
No. *Ss*	37	84	88	51	130	95
Means	25.8	30.3	30.0	28.7	28.4	29.1
σM	1.24	1.21	1.38	1.21	0.77	1.22

Tables I and II which give the mean scores for age and grade. One group of 95 *Ss* listed as ungraded were young male delinquents from an Industrial School, only a few of whom were in high school.

RESULTS

One of the main objectives of this study was to find the type of distribution of color perception among individuals. As stated by Haupt, "the general trend of opinion among experts in the field of color-blindness is towards the point of view that chromatic sensitivity varies from individual to individual in much the same way as do other physiological and psychological traits."[1] This study confirms that opinion, though there does seem

[1] I. A. Haupt, Tests for color blindness, *J. Gen. Psychol.*, 3, 1930, 244.

Frequency distributions of color perception are given in the following studies: A. Schuster, Experiments with Lord Rayleigh's colour box, *Proc. Roy Soc.*, 48, 1890, 140; R. A. Houston, A statistical survey of colour vision, *ibid.*, 94A, 1918, 576; R. A. Houston and M. Dunlop, A statistical survey of the colour vision of 1000 students, *Phil. Mag.*, 41, 1921, 186-200; M. Winfield and C. Strong, The Hering color-blindness apparatus and the normal Rayleigh equation, this JOURNAL, 32, 1921, 425-

to be a slight grouping of scores at the lower end of the scale. The frequency distribution of the total errors for three trials (Fig. 1) is skewed and may be fitted to a Pearson Type III curve with constants as follows:

$N = 485$,	Mean $= 29.08$,	$\sigma_{Dis.} = 10.65$,	$\sigma_M = 0.484$,
$B_1 = 1.27$,	$B_2 = 5.15$,	$K_1 = 0.55$,	$K_2 = 2.50$,
$\alpha_4 = 1.12$,	$\chi^2 = 9.167$,	$(n-1) = 9$,	$P = 0.43$.

Owing to the skewness of the data decile scores are given in Table III as norms for male *Ss*. Cumulative frequency distributions were constructed for

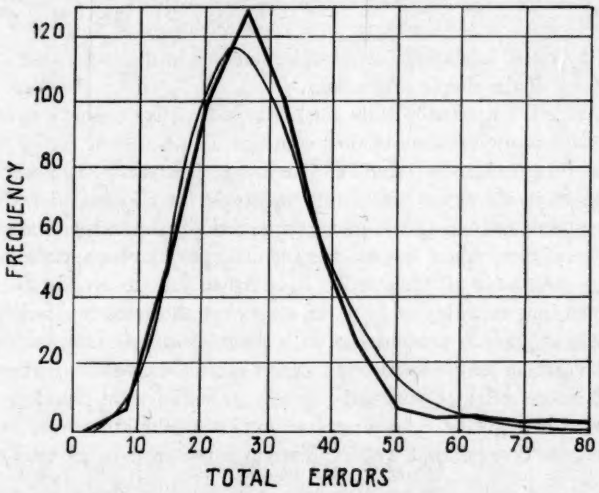


FIG. 1. FREQUENCY DISTRIBUTION OF THE ERRORS MADE BY 485 *Ss* (The smooth curve is a Pearson Type III curve.)

the total group and for every age and every grade, but are not shown here. There is a remarkably close agreement between all the curves within the limits of the probable error. It is to be noted (see Table I) that there is a wide variability at each age but that the mean score does not vary much with age within the range studied (age 10 yr. to adulthood).[2] The discriminative ability required for this test must on the average reach a

428; Istar Haupt, The Nela test for color blindness applied to school children, *J. Comp. Psychol.*, 6, 1926, 291-302; M. Collins, The Rayleigh colour equation with rotating discs, *Brit. J. Psychol.*, 19, 1929, 387-394; and R. A. Houston, *Vision and Colour Vision*, 1932, 193-207.

[2] The median error score for this group of boys is 28. Recently the median error score for a group of college men has been found to be 23. Thus there is a slight improvement with age up to college level.

maximum on or before 10 yr. of age. As a consequence one would expect little or no correlation with I.Q. or Mental Age. Such is indeed the case. The correlation between Binet-Stanford I.Q. and total errors for 98 *Ss* was —0.102 ± 0.067, and for another group of 35 *Ss* the correlation between Terman Group M.A. and total errors was 0.18 ± 0.11. The major rôle

TABLE III

DECILE SCORES FOR THE TOTAL GROUP
(N = 485 boys)

Deciles	1	2	3	4	5	6	7	8	9	10	95%
Score	—	18	21	24	26	28	31	33	37	42	48

played by verbal intelligence seems to be confined in this case to the comprehension of the simple instructions.

Since age is not a factor in the total error score it is of some interest to see if there is any variation of time with age. This was done for all three trials and is presented in Table IV. The youngest *Ss* take the longest times and the 14-yr.-old *Ss* are consistently the fastest for all three trials. Most of the differences from age to age are not statistically significant. The curvilinear correlation values between age and time per trial are approximately 0.30 ± 0.027 for all three trials. It is rather hard to account for the minimum time occurring at 14 yr. of age except on the basis of selection. The ratio of grade *Ss* to highschool *Ss* is about two to one. Only the duller pupils remain in the grades beyond 15 yr. of age, and hence the ages beyond 15 are rather weighted with duller pupils. It would seem therefore that Mental Age might be a factor, not in improving the error score but in reducing the time per trial. This explanation, however, does not seem com-

TABLE IV

TIME-SCORES (IN SEC.) ACCORDING TO AGE IN YEARS

Age	10 and 11	12	13	14	15	16	17, 18, 19	Totals
M_1	172	160	141	127	135	134	140	142
M_2	145	135	130	110	118	113	121	125
M_3	135	126	121	100	111	107	105	115
σM_1	6.3	4.4	4.7	4.4	4.2	3.9	5.2	1.8
σM_2	5.1	4.3	4.6	3.8	4.0	3.8	4.5	1.7
σM_3	6.8	4.0	4.6	3.5	3.8	3.5	4.3	1.6

plete; it is possible that at 14 yr. of age the retarding influences of the motor and perceptual slowness of the younger *Ss* and the caution of the older *Ss* is at a minimum.

When the times of those in the various quartiles of the total error score are separately averaged there is little or no difference between Q_2, Q_3, and Q_4 for all the trials. The times of Q_1 for all trials run, however, about

6% more than the average. Possibly the extra caution reflected in the additional time taken by the best *Ss* helps to improve their score. On the other hand there is no correlation between total time and total errors—the Pearson product-moment value for a group of 210 *Ss* is 0.05 ± 0.05.

Reliability. It is impracticable to determine the reliability of the test by repeating it or by giving an alternate form. In the first case practice

TABLE V

MEAN ERRORS PER TRIAL
(P.E. of the Means are about 0.14)

Group	First trial	Second trial	Third trial
Q_1	6.4	5.6	5.3
Q_2	9.0	8.0	7.6
Q_3	11.2	10.2	9.3
Q_4	16.2	14.5	13.0
Total	10.7	9.6	8.3

effects are too noticeable and in the second case the material was not available. The reliability may be approximated by the use of the Spearman-Brown formula, based on the intercorrelations of the errors of the three trials, which were as follows for a group of 210 *Ss*: $r_{12} = 0.51 \pm 0.03$; $r_{23} = 0.48 \pm 0.04$; $r_{13} = 0.36 \pm 0.04$. From which we obtain the reliability, $r_{11} = 0.76 \pm 0.01$.[3]

When the time scores for the three trials were intercorrelated for the same group of *Ss* the following values were found: $r_{12} = 0.670 \pm 0.026$, $r_{23} = 0.660 \pm 0.027$, $r_{13} = 0.756 \pm 0.020$; from which we obtain,

TABLE VI

PERCENTAGE OF ERRORS MADE ON EACH TYPE OF CARD

No. stimulus-spots	% predom. color (y)	Percentage of errors (x)				
		Q_1	Q_2	Q_3	Q_4	Total
18	50.0	1.0	1.7	2.7	8.5	4.4
16	44.5	4.8	6.9	9.4	13.8	9.9
14	39.0	10.6	14.8	15.7	17.9	15.6
12	33.3	29.6	28.0	28.0	25.2	27.2
11	30.5	48.8	48.8	44.3	34.5	43.0
Equations		$y=56/x^{0.15}$	$y=68/x^{0.21}$	$y=78/x^{0.25}$	$y=120/x^{0.39}$	$y=78/x^{0.35}$
χ^2		3.38	2.23	1.94	0.27	0.59
P		0.63	0.81	0.85	0.99	0.98

$r_{11} = 0.88 \pm 0.01$. Thus the test is more reliable for time than it is for errors. Table V lists the mean errors per trial for each quartile. On the average there is about a 10% reduction of errors (and of time also, cf. Table IV) from trial to trial.

Frequency of errors. As was explained in the section dealing with the

[3] J. P. Guilford, *Psychometric Methods*, 1936, 419.

test material the numbers of dots of the predominating color may be 18, 16, 14, 12, or 11. As the total number of dots on all cards is 36 the percentages the predominating color is of the total coloring are respectively:

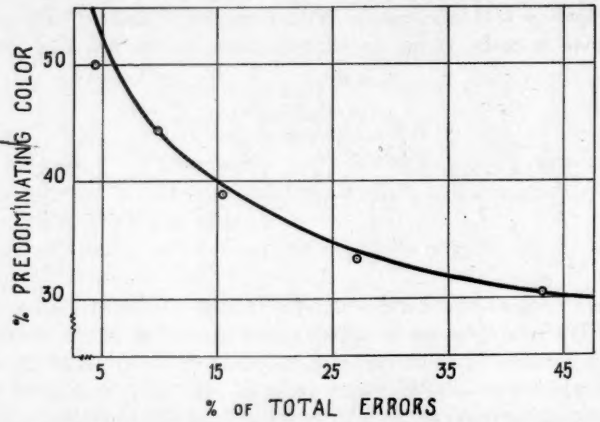


FIG. 2. CURVE SHOWING THE FUNCTIONAL RELATIONSHIP BETWEEN THE PERCENTAGE THE PREDOMINATING COLOR IS OF THE TOTAL COLOR, PER TYPE OF CARD (y) AND THE PERCENTAGE OF ERRORS MADE ON THAT CARD (x)
(The equation of the curve is $y=78/x^{0.25}$)

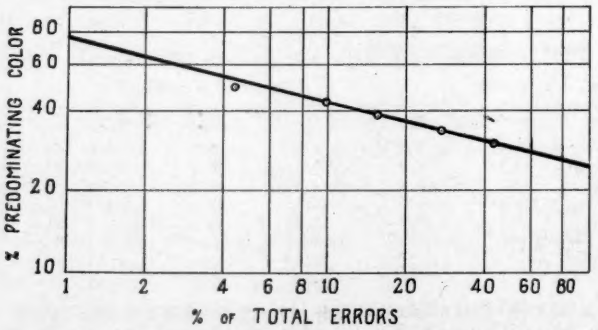


FIG. 3. CURVE SHOWING THE RELATIONSHIP BETWEEN PREDOMINATING COLOR AND PERCENTAGE OF ERRORS
(Data plotted on logarithmic paper)

50.0, 44.5, 39.0, 33.3, and 30.5. Table VI gives the frequency of the errors per type of card tabulated in terms of the percentages of the total errors made on all the cards, and differentiated according to the quartile

of the distribution. There seems to be a functional relationship between the two variables as expressed in the equations given in the same table and shown in the curves of Figs. 2 and 3. This relationship may be stated as follows. The difficulty of a card, as determined by the percentage of errors made on that card, is a logarithmic function of the relative mass of the predominating color of the card. As the exponents of the determined variable and the constants of the equations increase progressively with the

TABLE VII

PERCENTAGE FREQUENCY OF ERRORS ACCORDING TO PREDOMINATING COLOR

Colors	Quartile				Percentile	Total
	1	2	3	4	95	
Blue	45.5	43.7	43.5	36.0	32.2	41.4
Green	12.2	12.0	12.2	17.8	21.7	14.2
Purple	22.4	23.8	26.1	26.5	25.7	25.0
Red	19.8	20.3	18.2	20.3	20.5	19.4

quartiles the relative difficulty of any card varies with the ability of the *Ss*.

The quartile rank of the *Ss* must also be taken into account in determining the difficulty of the different predominating colors. Table VII lists the errors made when cards of different predominating colors are sorted. It is seen that blue is about four times as difficult (measured in percentages of the total number of color confusions) as green, for the best quartile, but only about twice as difficult for the poorest quartile. The purples and the reds do not vary much in difficulty. The very poorest *Ss* (percentile rank 95 and over) find about the same difficulty for all four colors, though the blues are still the hardest and the greens the easiest.

TABLE VIII

PERCENTAGE OF CARDS ERRONEOUSLY THROWN INTO EACH COMPARTMENT

Compartments	Quartile				Percentile	Total
	1	2	3	4	95	
Blue	8.4	9.4	11.9	19.8	25.6	13.8
Green	34.1	37.3	37.7	29.9	22.5	34.4
Purple	45.9	41.8	38.4	31.0	34.2	39.4
Red	11.5	11.0	11.9	14.0	17.8	12.4

When the errors are analyzed, as in Table VIII, by tabulating the compartments into which the colors are wrongly thrown, we are in a position to gauge the confusion colors. It is evident that cards are rarely confused for blue or red, and errors are most frequently made by confusing a card for purple or green. This mistake is relatively most frequently made by the best quartile. The explanation may be partly due to the fact that the poorest judges of color are so confused that they throw cards almost at

random into any of the compartments, and chance sorting of this nature would tend to make all the errors of equal frequency for each compartment. Once again it must be emphasized that the difficulty of a card or the frequency of an error can only be ascertained when the quartile of the distribution of the scores is known.

TABLE IX

PERCENTAGE OF CONFUSION ERRORS PER COLOR, AND FOR THE ENTIRE TEST

Confusion colors	Errors per color				Errors of entire test			
	Blue	Green	Purple	Red	Blue	Green	Purple	Red
Blue	—	27	28	17	—	3.8	6.3	3.1
Green	30	—	54	44	12.4	—	13.5	8.6
Purple	59	51	—	39	24.4	7.4	—	7.6
Red	11	22	18	—	4.5	3.1	4.5	—

Table IX gives in the left half of the table the percentage of times that the various confusion colors occur for each color, and in the right half the percentage of all errors made by mistaking each color with its confusion colors. It is to be noted that nearly one quarter of all mistakes is made by calling the blue cards purple; also that red and blue rarely act as confusion colors for the others. Green and red are least often erroneously sorted.

DISCUSSION

An attempt was made to keep intensity, saturation, and visual angle constant and to set a limit on time. The printed colors were individually matched with black and white on the color wheel with fairly uniform results. When the colors were printed on the cards as in the test the variations in saturation and brightness were considerably increased due to a number of factors.

(a) Since brilliance is the arithmetic sum of the photometric values in pure photopic vision, the brilliance of the cards varies according to the relative amounts of the colors on each card.[4]

(b) Spacial induction differs from card to card owing to the random arrangement of the colors, with a consequent change of luminosity.[5]

(c) Hue and saturation are markedly affected by the size of the stimulus. Matchings made when the stimuli were fairly large (about 23° visual angle on the color wheel) are not valid when the stimuli were reduced, as they were for the individual dots, to about 1°.[6]

[4] H. E. Ives, The addition of luminosities of different colors, *Phil. Mag.*, 32, 1912, 845-853.

[5] J. H. P. Parsons, *An Introduction to the Study of Colour Vision*, 2nd ed., 1924, 140.

[6] L. H. Troland, Visual phenomena and their stimulus-correlations, *Foundations of Experimental Psychology*, 1929, 185, 198.

(d) The protochromatic interval, the intensity range over which the saturated hues tend to appear gray, varies for the different colors; thus blue, for instance, is considerably more affected than is red. The size of the dots comes well within the range where this effect is most pronounced.[7]

(e) The fatigue effects vary for different colors and become noticeable after a fairly long series of judgments.[8]

(f) Chromatic adaptation or minuthesis, the diminution in saturation of a hue as one continues to observe it, is also apparent.[9] Consequently it is not possible to hold the factors of intensity and saturation rigidly constant in this test. There are variations not only from card to card, but also from *S* to *S*. These factors, therefore, tend to produce the results mentioned in the section on color confusions. The complexity of the factors complicates the answer to the question: Just what is being measured by this test? Certainly not intensity alone, nor saturation, nor chroma, but the total color perception of a heterogeneous mass of color, in which the elements are variously distributed and mutually affect each other.

It follows that this test might be utilized somewhat after the manner of the pseudo-isochromatic tests, like Ishihara's, Nagel's, and Stilling's, to measure color blindness. An investigation with this in view is now in progress and so far the results seem to support this view. Too few cases, however, have as yet been studied to enable one to generalize with any degree of assurance.

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[7] Troland, *op. cit.*, 197; Parsons, *op. cit.*, 133.
[8] Parsons, *op. cit.*, 128.
[9] D. McGregor, Sensitivity of the eye to saturation of color, *J. Exper. Psychol.*, 19, 1936, 525-547.

THE RELATION BETWEEN PSYCHOSIS AND PHYSICAL TYPE; A STATISTICAL STUDY

By E. FRANCES WELLS, Nyack, N.Y.

According to Kretschmer, there is a positive correlation between major forms of psychosis and certain bodily characteristics. Thus dementia praecox is said usually to occur in persons of esthenic (leptosome) build and the manic-depressive states in those of the pyknic type. The correlation is set as high as +0.96. Athletic and esthenic-athletic mixed types tend toward schizophrenia, while the pyknic mixed forms tend to develop circular disorders. When these intermediate types are included, the coefficient drops to +0.93.[1] This psychophysical relationship is supposed to hold for normal individuals as well as for the insane, the personality of the normal esthenic being described as *schizothymic* and that of the pyknic as *cyclothymic*.[2]

Kretchmer's theory of constitutional types has been generally supported by a number of studies. Functional differences between the two extreme types (esthenics and pyknics) have been demonstrated by several investigators. Van der Horst, working with normal and insane subjects, found striking differences between the two types in performance on objective tests, such as tapping, reaction-time, range of attention, and color fusion.[3] Kibler, Wertheimer and Hesketh, Enke, and Kroh also obtained results which tend to support the theory.[4] More recently, specific differences in affectivity, motor functions, and skin sensitivity have been discovered by Enke, Meerowitsch, and Fricke.[5] The extensive typological studies of Jaensch, representing

* Accepted for publication June 15, 1936. The material for the study was obtained at Danvers State Hospital, Massachusetts, during the summer of 1930. The author is indebted to Dr. Clarence Bonner for providing facilities and to Dr. Grace Kent for help in selecting cases and interpreting records.

[1] E. Kretschmer, *Körperbau und Charakter*, 8th ed., 1929, 33.

[2] *Op. cit.*, 170-172.

[3] L. van der Horst, Experimental-psychologische Untersuchung zu Kretschmer's "Körperbau u. Charakter," *Zsch f. d. ges. Neur. u. Psychiat.*, 93, 1924, 341-380.

[4] M. Kibler, Experimentalpsychologische Beitrag zur Typenforschung, *ibid.*, 98, 1925, 524-544; F. Wertheimer and F. E. Hesketh, A minimum scheme for the study of the morphological constitution in psychiatry with remarks on anthropometric technique, *Arch. Neur. u. Psychiat.*, 17, 1925, 93; and *The Significance of the Physical Constitution in Mental Diseases*, 1927, 1-87; W. Enke, Die Konstitutionstypen im Rohrschachsen Experiment, *Zsch. f. d. ges. Neur. u. Psychiat.*, 108, 1927, 645-674; and Experimentalpsychologische Studien zur Konstitutionsforschung, *ibid.*, 114, 1928, 720-794; O. Kroh, Experimentelle Beiträge zur Typenkunde, *Zsch. f. Psychol., Ergbd.*, 14, 1929, 22; and *Experimentelle Beiträge zur Typenkunde*, 1932, 1-356.

[5] W. Enke, Die Affektivität der Konstitutionstypen im psychogalvanischen Versuch, *Zsch. f. d. ges. Neur. u. Psychiat.*, 138, 1932, 211-237; W. Enke and B. Meerowitsch, Experimentelle Untersuchungen Psychomotorik der Konstitutionstypen und ihre Beeinflussung durch exogen Faktoren, *ibid.*, 147, 1933, 194-215; F. Fricke, Persönlichkeitstypus und Gewebsfunktion, *Zsch. f. Psychol.*, 134, 1935, 62-99.

a different approach to the problem, have indirectly strengthened Kretschmer's position.[6]

On the other hand, several studies designed to test the theory have produced negative results. Mohr and Gundlach, studying a group of male convicts, found by testing only slight differences between the two physical types in intelligence and performance. Their results show a general gradation both in performance and in physical characteristics rather than a sharp division into types.[7] Garrett and Kellogg, in a study of the relationship between physical constitution and general intelligence, social intelligence, and emotional stability, likewise came to a negative conclusion.[8] Campbell,[9] attempting to verify Kretschmer's correlation by measurement of a large group of state hospital patients and Garvey,[10] with a smaller group, also failed to establish a correlation. Wigert found a positive relation between the leptosome type and schizoid temperament, but none between the pyknic type and manic-depressive disorders.[11] An experimental study of 232 college students by Klineberg, Asche, and Block revealed no evidence of distinct psychological types.[12] Hamilton and Shock report no consistent relation between morphological indices and measures of personality.[13]

Kretschmer's theory has also been attacked on logical grounds by a number of critics, among whom may be mentioned Mollenhoff, Klüver, Kolle, Huth, and Heun.[14] These critics generally agree that the theory is unsound but that it has,

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[6] W. Jaensch, Über Psychophysische Konstitutionstypen, *Münch. med. Woch.*, 68, 1921, 1101; E. R. Jaensch, Über Methoden der pyschologischen Typenforschung, *Zsch. f. Psychol.*, 108, 1928, 1-16; Studien zur Psychologie menschlicher Typen, *ibid.*, 109, 1929, 241-290; 110, 51-112; 111, 1-145; 112, 139-232; 113, 91-181; Das Verhältnis der Integrationstypologie zu anderen Formen der Typenlehre, insbesondere zur Typenlehre Kretschmers, *ibid.*, 125, 1932, 113-148; Zur Auseinandersetzung der Typenlehre Kretschmers und der Integrationstypologie, *ibid.*, 130, 1933, 370-385.

[7] G. J. Mohr and R. H. Gundlach, The relation between physique and performance, *J. Exper. Psychol.*, 10, 1927, 117-157; A further study of the relation between physique and performance in criminals, *J. Abn. & Soc. Psychol.*, 24, 1929, 91-103.

[8] H. E. Garrett and W. N. Kellogg, The relation of physical constitution to general intelligence, social intelligence and emotional instability, *J. Exper. Psychol.*, 11, 1928, 113-129.

[9] J. K. Campbell, The relation of the types of physique to the types of mental disease, *J. Abn. & Soc. Psychol.* 27, 1932, 147-151.

[10] C. R. Garvey, Comparative body build of manic-depressive and schizophrenic patients, *Psychol. Bull.*, 30, 1933, 567-568.

[11] V. Wigert, Versuche zur anthropometrischen Bestimmung der Körperbautypen, *Zsch. f. d. ges. Neur. u. Psychiat.*, 143, 1933, 651-700; Attempts at anthropometric determination of the body types of Kretschmer, *Acta psychiat. et neur.*, 8, 1933, 465-481.

[12] O. Klineberg, S. E. Asche, and H. Block, An experimental study of constitutional types, *Genet. Psychol. Monog.*, 16, 1934, 145-221.

[13] J. A. Hamilton and N. W. Shock, An experimental study of personality, physique and the acid-base equilibrium of the blood, *Amer. J. Psychol.*, 48, 1936, 467-473.

[14] F. Mollenhoff, Zur Frage der Beziehungen zwischen Körperbau und Psychose, *Arch. f. Psychiat. u. Nervenkr.*, 71, 1924, 98-127; H. Klüver, An analysis of recent work on the problem of psychological types, *J. Nerv. & Ment. Dis.*, 62, 1925, 561-596 (*esp.* 576-584); Do personality types exist?, *Amer. J. Psychiat.*, 10, 1931, 781-788; and Typological method, E. Kretschmer's study of physique and character, in *Methods in Social Science*, (ed. by S. A. Rice), 1931, 176-185; K. Kolle,

nevertheless, some practical value. On this basis, it is being accepted and applied in clinical diagnosis by many psychiatrists.[35]

The present study represents another attempt to test Kretschmer's theory. Specifically, we sought (1) to find out whether there are statistically reliable differences in physical build between a group of 50 unselected dementia praecox patients and a like group of manic-depressives, (2) to classify these patients as to physical type, (3) to correlate the types with psychoses, and (4) to determine the relationship between physical type and factors of age and race.

The *Ss* were 100 adult male patients at Danvers State Hospital in Massachusetts.[36] They were selected on the basis of time of admission and diagnosis. Only those who had a clear-cut diagnosis of dementia praecox or manic-depressive psychosis, supported by case-histories, were included.[37] Patients who had left the hospital or who were too ill to be brought to the examining room were necessarily omitted. The two groups finally chosen represent successive admissions over a period of 1 to 2 years, subject to these limitations.

Physical measurements were made of all the subjects.[38] Height was measured in inches, weight in pounds, and all other dimensions in centimeters. Measurements were made accurately to the nearest half-centimeter or quarter-inch. Greater precision was not attempted because slight inaccuracies due to errors of observation could not be eliminated by our technique. If there is a significant difference between the two groups, it must be considerably more than ¼-cm. or ⅛-in. which is the limit of error in our method. Three essential ratios—weight-height, shoulder-chest and cephalic index—were computed.

Analysis of Data

Differences in physical build. Statistical analysis of our measurements reveals characteristic differences between the two groups. The manic-depressive (M.D.) is, on the average, slightly shorter and heavier than

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Klinische Beiträge zum Konstitutionsproblem, *Arch. f. Psychiat. u. Nervenkr.*, 77, 1926, 183-238; 78, 1927, 93-164; A. Huth, Das Ende der Typologie, *Indus. Psychotechn.*, 10, 1933, 383-384; E. Heun, Kretschmers Lehre vom Körperbau und Charakter, *Zsch. Menschenk.*, 10, 1935, 215-229.

[35] For a more detailed summary of recent work on the subject, see H. Klüver, An analysis of recent work on the problem of psychological types, *op. cit.*,; J. B. Maller, Studies in character and personality in German psychological literature, *Psychol. Bull.*, 30, 1933, 209-232; and O. Klineberg, *op. cit.* Bibliographies are given by these authors.

[36] Only male patients were used because, according to Kretschmer, physical types are more clearly evident in men than in women.

[37] Many cases were rejected because their case-histories cast doubt upon the accuracy of the diagnosis. For the exceedingly high percentage of disagreement in diagnosis of schizophrenia in Massachusetts state hospitals, see the volume *Schizophrenia; Statistical Studies from the Boston Psychopathic Hospital, 1925-1934*, (reprints), Reprints 2, 3, and 11.

[38] The measurements correspond to those given by Kretschmer, (*op. cit.*, 17, 45), except that measures of the extremities and face were omitted.

the dementia praecox individual (D.P.), and his abdominal girth is a little larger (Table I). The shoulder, chest and cranial measurements, however, spoil the 'type' picture. According to theory, the M.D. individual should be predominantly pyknic, with broader shoulders, larger chest and higher cephalic index than the esthenic D.P.;[19] but, with our *S*s, these dimensions are smaller in the M.D. Thus our averages fail to support Kretschmer's theory of types, although they do show slight differences between the

TABLE I

STATISTICAL MEASURES

	Measures	Av.	A.D.	S.D.	P.E.$_{av}$	Range	Q_1	Med.	Q_3
Dementia praecox group	Wt. (lb.)	144.73	14.75	18.56	1.76	103.50–199.50	134.25	140.75	157.25
	Ht. (in.)	67.06	1.72	2.16	.20	63.00– 72.00	65.75	67.25	68.00
	Wt./Ht.	2.15	.21	.26	.02	1.64– 2.81	1.98	2.11	2.34
	Shoulders	42.83	1.89	2.36	.22	37.00– 48.00	41.00	43.00	44.00
	Chest	93.30	4.71	5.87	.56	83.00–111.00	89.00	92.50	96.00
	Sh./Ch.	.46	.02	.02	.00	0.40– 0.52	0.44	0.46	0.47
	Abdomen	85.53	7.13	8.94	.85	67.00–106.00	78.00	84.50	91.00
	Head (wd)	14.78	.44	.61	.05	13.70– 16.50	14.50	14.80	15.00
	Head (lg)	18.95	.46	.64	.06	16.80– 20.30	18.80	19.00	19.50
	Ceph. Ind.	.78	.03	.03	.00	0.71– 0.87	0.74	0.78	0.80
	Head (cir)	56.37	1.34	1.70	.16	51.50– 61.00	55.30	56.50	57.50
Manic depressive group	Wt. (lb)	147.22	15.88	21.49	2.04	94.25–206.00	135.00	145.38	156.50
	Ht. (in.)	66.70	2.16	2.70	.25	60.00– 73.00	65.00	66.88	68.25
	Wt./Ht.	2.20	.22	.29	.02	1.57– 3.01	2.00	2.21	2.36
	Shoulders	41.69	1.69	2.12	.20	36.50– 45.00	40.00	42.00	43.00
	Chest	92.84	5.12	6.34	.60	77.00–106.00	88.00	93.50	96.50
	Sh./Ch.	.45	.02	.02	.00	0.38– 0.51	0.43	0.45	0.47
	Abdomen	86.65	7.15	9.86	.94	67.50–120.00	79.00	86.00	91.00
	Head (wd)	14.88	.40	.53	.05	13.90– 16.20	14.50	14.85	15.20
	Head (lg)	19.13	.53	.75	.07	17.50– 22.00	19.00	19.00	19.50
	Ceph. Ind.	.77	.02	.03	.00	0.66– 0.84	0.76	0.78	0.81
	Head (cir)	56.45	1.46	1.81	.17	52.20– 60.50	55.00	57.00	57.50

groups. These differences, however, are statistically unreliable. A comparison of the ranges of distribution will show broad overlapping in every case. For example, the range for weight among the M.D. is 94.25–206 pounds, the middle 50% falling between 135 and 156.5 pounds, while among the D.P. the range is 103.5–199.5, with the middle 50% at 134.25–157.25. When S.D. and P.E. of the averages are considered, the differences are without significance. For example, S.D. for weight stand 21.496 and 18.565 (showing a very high variability) while P.E. of 2.048 and 1.769 indicate that the true averages actually overlap. The limits of

[19] Kretschmer, *op. cit.*, 17, 26, 45, 55. The cranial measurements of the two types, as given by Kretschmer, show a higher cephalic index for the esthenic schizophrenics than for the pyknic circulars. But he describes the esthenic skull as short and of middling breadth, the pyknic as large and round, implying a higher index for the latter. Anthropological evidence supports this description.

the true averages of all the measures are given in Table II. The overlapping, which occurs in every case, shows that there are no reliable differences.

The P.E. of the differences give further proof that the obtained differences are unreliable.[20] To insure complete reliability, $D/P.E._{diff.}$ must be at least 4.0. Since all our $D./P.E._{diff.}$ are less than 4.0, none of the differences are reliable (Table III). In one case (shoulders) there is a close approach to complete reliability ($D./P.E._{diff.} = 3.77$), the chances of a true difference being 99 in 100. There is no absolutely reliable difference between the two groups.

TABLE II

RANGES OF TRUE AVERAGES

Measures	Dementia praecox group	Manic depressive group
Weight	137.65–151.81	139.03–155.41
Height	66.23– 67.88	65.57– 67.73
Wt./Ht.	2.05– 2.25	2.10– 2.31
Shoulders	41.93– 43.73	40.88– 42.49
Chest	91.06– 95.53	90.42– 95.53
Sh./Ch.	.45– .47	.44– .46
Abdomen	82.12– 88.93	82.88– 90.41
Head (wd)	14.54– 15.01	14.67– 15.08
Head (lg)	18.71– 19.19	18.84– 19.42
Ceph. Ind.	.76– .79	.76– .79
Head (cir)	55.71– 57.02	55.75– 57.14

TABLE III

PROBABILITIES OF TRUE DIFFERENCES

Measures	Diff.	P.E.$_{(diff.)}$	Chances of true diff.
Weight	+2.49	2.70	73 in 100
Height	− .35	.33	76 in 100
Wt./Ht.	+ .04	.02	87 in 100
Shoulders	−1.14	.30	99 in 100
Chest	− .46	.82	64 in 100
Sh./Ch.	− .01	.004	96 in 100
Abdomen	+1.12	1.26	73 in 100
Head (wd)	+ .10	.07	83 in 100
Head (lg)	+ .18	.09	91 in 100
Ceph. Ind.	− .002	.005	61 in 100
Head (cir)	+ .08	.23	59 in 100

Classification according to type. If there are two or three distinct types of physique, as Kretschmer holds, one might expect a priori that graphical representation of the physical measures would yield bi-modal or tri-modal curves, which would furnish an objective basis for classification. The curves

[20] The differences are obtained by subtracting the averages for the D.P. from those of the M.D. group. A plus difference means that the M.D. average is larger, and vice versa. The chances of a true difference (last column) are taken from Garrett, *Statistics in Psychology and Education*, 1926, 135.

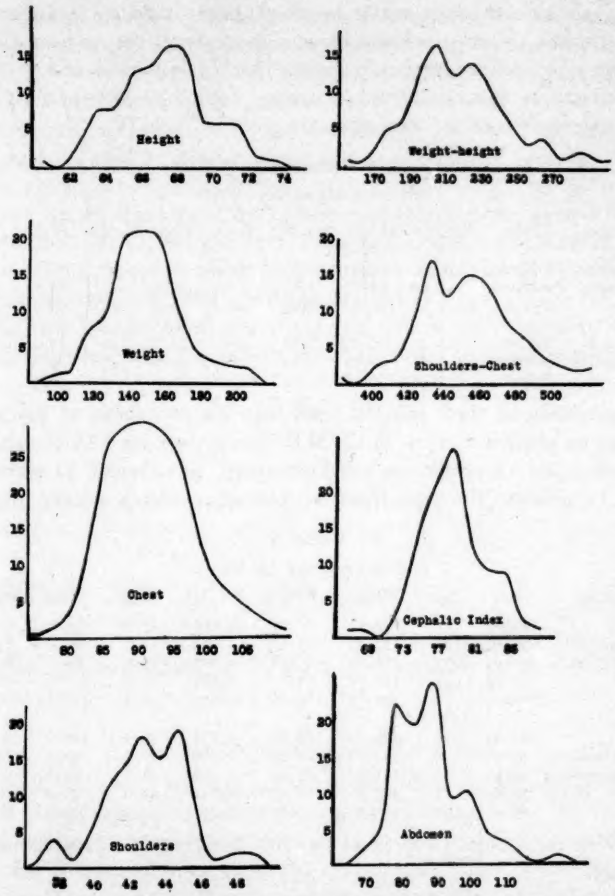

FIG. 1. DISTRIBUTION OF PHYSICAL CHARACTERISTICS IN 100 *Ss*
(50 Manic depressives and 50 Dementia praecox)

of distribution for our data vary in form; four are bi-modal, pointing to the existence of two distinct types and four approximate the normal bell-shaped curve (Fig. 1).[21]

Since the graphs thus fail to supply an unequivocal basis for determining

[21] Curves were not plotted for the cranial measurements.

types, we have classified our *Ss* by weight-height ratio alone, following Kretschmer's system: the lower ratios, esthenic type; the medium ratios, athletic type, and the higher ratios, pyknic type.[22] *Ss* with ratios of 1.55–1.99 are classed as esthenic, 2.00–2.29 athletic, and 2.30–3.00 pyknic. The average measures of the three types are given in Table IV.

TABLE IV

AVERAGE MEASURES OF TYPES

Group	Type	Weight	Height	Wt./Ht.	Shlds.	Chest	Sh./Ch.	Abd.	Ceph. Ind.
Dementia praecox	Esthenic	124.70	66.41	1.87	41.4	88.6	.46	78.0	.79
	Athletic	143.20	67.60	2.11	43.2	93.3	.46	85.2	.76
	Pyknic	167.18	66.86	2.49	43.7	97.9	.44	93.7	.79
Manic depressive	Esthenic	123.40	66.29	1.85	40.0	86.7	.46	77.4	.78
	Athletic	143.86	66.67	2.15	41.7	91.9	.45	84.8	.77
	Pyknic	171.45	67.08	2.55	43.0	99.3	.43	96.9	.78

Correlation of these physical types with the two forms of psychosis shows no positive relation. In the M.D. group, there are 12 esthenics, 23 athletics, and 15 pyknics; in the D.P. group, 14 esthenics, 22 athletics, and 14 pyknics. The three types are thus approximately equally divided between the groups. Only 51 of the 100 cases run true to Kretschmer's theory.

TABLE V

MEDIANS FOR FIVE AGE-GROUPS

Group	Age	No.	Weight	Height	Wt./Ht.	Shlds.	Chest	Abdomen
Dementia praecox	20–29	11	140.50	67.75	2.155	43.0	93.0	85.0
	30–39	23	137.50	67.50	2.029	42.0	90.5	81.0
	40–49	11	151.00	66.25	2.215	43.0	95.5	87.0
	50–59	3	150.75	67.0	2.390	44.0	102.0	98.0
	60+	2	175.75	69.13	2.530	45.5	101.3	97.0
Manic depressive	20–29	3	169.00	67.00	2.522	43.0	100.0	91.0
	30–39	8	139.50	66.90	2.088	42.0	90.3	81.5
	40–49	10	145.80	65.00	2.225	42.0	92.0	82.0
	50–59	15	146.8	67.0	2.234	42.0	96.0	87.5
	60+	14	139.5	66.0	2.050	41.0	93.0	86.5

Relation between physical type and age. Kretschmer holds that his physical types persist and are recognizable in spite of the bodily changes which occur with age, that is to say that the types are constant throughout life.

Since it was not possible for us to measure our *Ss* at different ages, we

[22] Kretschmer, *op. cit.*, 17, 21, 26. The averages given by Kretschmer are: *esthenic*, weight 50.5 kg., height 168.4 cm.; *athletic*, 62.9 kg., 170.0 cm.; *pyknic*, 68.0 kg., 167.8 cm. Translated into English equivalents, these measures give weight-height ratios of 1.679, 2.072 and 2.269 respectively.

cannot check this theory directly. But, by fractionating our data according to age-groups, the relation between age and physical type can be demonstrated. *Ss* were divided into five age-groups, each with a range of 10 yrs. Medians for these groups are given in Table V.[23]

The fractionated measures show definite variations with age. Weight-height ratios tend to increase with age in both classes of patients at least up to 60 yrs. Chest and abdominal measures show similar tendencies. In no case do the measures of the different age-groups center about a norm, higher for one class than the other, as logically demanded by Kretschmer's theory. Thus the theory of constancy of type is not supported by our data. Age is an important factor in determining type.

Relation between physical type and race. Our *Ss* are classified by racial groups in Table VI.[24] No definite relation between race and physical type

TABLE VI

RACIAL CLASSIFICATION

Group	Type	Celt.-German	Mediter.	Negro	Semitic	Slavic	?	Totals
Dementia praecox	Esthenic	6	3		2	2		13
	Athletic	17	1	1	1		1	21
	Pyknic	7	2	2	2	3		16
Manic depressive	Esthenic	8	3			1		12
	Athletic	19	3		1			23
	Pyknic	11	2		1		1	15

is apparent. On the contrary, the three types are fairly evenly distributed, for the most part, in each race-group.

DISCUSSION OF RESULTS

Up to this point, we have taken for granted as a working hypothesis the theory of types. The concept requires critical analysis.

As applied to human personality, the term 'type' ordinarily denotes the extreme variation of some particular characteristic. Thus, a person is of the 'intellectual type' if he shows intense interest in intellectual matters. In biology, 'type' signifies either a general form of structure common to a number of individuals or the most perfect representation of the character-

[23] Medians are given instead of averages because of the small number of cases in each group and the relatively high variability. The statistical reliability is of course low.

[24] This classification is only approximate, as it was impossible to secure complete and reliable racial histories in every case. Celtic and Germanic races have been included in one group because of a number of cases of racial mixture due to intermarriage.

istics common to a group.[25] Like common sense, biology regards the extreme-variant as the most typical. In statistical psychology, the term 'type' has come to be used in the sense of a central form or average, about which variations center. A psychological 'type specimen' represents the mean of a group.

Kretschmer uses the term in both the ordinary-biological and the psychological senses. Type, he says, is a purely empirical concept, denoting average values, not an ideal a priori construct. He explains thus the method of determining types: "When a fairly large number of morphological similarities can be followed through a correspondingly large number of individuals, then we pause and register the measurement. From this we reckon the average value, and thus the outstanding common characteristics come out clearly. . . . Only those characteristics which become strongly marked in the average values are described as typical."[26]

After defining the term 'type' in this way, Kretschmer inconsequentially states that his *description of types refers,* not to the average or most frequent cases but to *ideal cases,* which bring out most clearly the characteristics of the group.[27] Although Kretschmer thus reverts to the common-sense-biological usage, his theory definitely implies the existence of distinct type-groups. The distribution curves for the physical characteristics, by which he defines his types, should, therefore, be multimodal in form, the number of modes corresponding to the number of types.

Our results do not substantiate this theory. Although four of our curves of distribution (Fig. 1) are actually bimodal, their general form suggests that the irregularity is due to the relatively small number of cases rather than to any real division into distinct types. The remaining four curves approximate the normal bell-shaped form.

These results indicate that types, in the psychological-statistical sense, do not exist. The persistent tendency to create type concepts is probably due to a confusion of the two senses of the term. Extreme variations in physical and mental characteristics are easily identified, but their existence in no way implies psychological type-groups.

Summary

(1) Statistical analysis of the physical measurements of 50 dementia praecox and 50 manic-depressive patients shows no reliable differences between the groups.

[25] *Webster's Collegiate Dictionary,* 1919, 1040.
[26] Kretschmer, *op. cit.,* 15. The English translation is taken from W. J. H. Sprott, (*Physique and Character,* by E. Kretschmer, 1925, 18-19).
[27] Kretschmer, *op. cit.,* 16.

(2) Kretschmer's theory of distinct physical types is not substantiated by our results.

(3) Kretschmer's correlation between bodily type and psychosis is not sustained by our analysis.

(4) Fractionation of our data into age-groups shows that age is an important factor in the determination of physical type.

(5) Fractionation into race-groups reveals a distribution of these types within each race but no demonstrable relation between race and type.

THE OPTIMAL MUSCULAR TENSIONS FOR VARIOUS PERFORMANCES

By G. L. FREEMAN, Northwestern University

Surveys of existing literature indicate that muscular tension may have facilitative or inhibitory effects upon performance, depending on circumstance.[1] The present report studies one influencing factor in the relationship; namely, the amount of muscular tension which is utilized in support of the test activity. In the laboratory, the writer has frequently observed that while increased effort and tension go hand in hand, they are not always effective in improving performance. Indeed, there is reason to believe that the more complex acts are definitely impeded by the presence of excessive tension in the behavior flux. Thus, while a certain increment in tension might improve a person's tapping ability, it would have a deleterious effect upon his golf score.

The problem of establishing the optional tension for a given performance is beset with methodological difficulties. (1) Different amounts of tension must be induced in muscle-groups which are remote enough from the performing member to offer no mechanical interference with its operation and yet near enough to produce a spread of excitation to that member. (2) The range of tension-loads must be sufficiently great to permit the occurrence of both inhibitory and facilitative effects. Furthermore, the tests must be so timed that performance accompanying one grade of tension will not be seriously influenced by residual effects of a previous tension load. (3) The performance must be influenced by the tensions which are induced experimentally and not by uncontrolled variations in other parts of the body. (4) The performance under survey must not show major practice or fatigue effects else these rather than the experimentally induced tensions will become the major determinants of work output. Careful study of these difficulties resulted in the procedures which follow.

METHOD AND PROCEDURE

The performances chosen for test were such as utilized the upper part of the body for sensory input and motor outgo and included finger oscillation, mirror-drawing, intensity limen for touch, eyelid reflex, and mental arithmetic. Most of the work was done on the first two performances named, with tensions induced in the lower limbs by the maintenance of various pressures. *Ss* lay on a comfortable cot and pushed with their feet against lever arrangements attached to ordinary spring balance scales. Mirror-drawing apparatus, finger-ergograph, threshold-determiner,[2] air-puff eyelid-stimulator and kymographic recorder were arranged so that all performances could be conveniently tested with *S* lying on his back. As a check upon the general excitation background of *S* we employed the palmar skin-resistance. This phenome-

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[1] Cf. G. L. Freeman, The facilitative and inhibitory effects of muscular tension upon performance, this JOURNAL, 45, 1933, 17-52; R. C. Davis, The relation of certain muscle action potentials to "mental work," *Ind. Univ. Publ.*, 1937, Science Series, No. 5.

[2] R. M. Simpson, An instrument for measuring intensity of pressure and pain stimuli, this JOURNAL, 49, 1937, 117-119.

non is being used extensively as an indicator of physiological reactivity or "energy mobilization" and has been shown by Freeman and Simpson to reflect quantitative increments of muscular tension.[3] The general method of inducing tension and recording palmar skin-resistance was the same here as in the study just mentioned; *S* first lay in the prone position on the cot and 'relaxed down' until his palmar skin-resistance showed no marked fluctuation (over 1000 ohms) for a period of 5 min. Then his feet were placed in stirrup supports before the lever arrangements and the palmar resistance was read at this 'zero' tension.[4] At intervals thereafter he was made to maintain scale-pressures reading 10, 20, 40, and 80 lb. respectively. Because of level advantage the pressures maintained were not as great as indicated by the readings. The pressures were equally divided between the two legs and the palmar skin-resistance was read under each condition. The *Ss* were ten undergraduate men who served in Procedures 1 to 4, and two graduate students who served in Procedure 5. All but the last two were completely naïve as to the purpose of the experiment. Maximal motivation was sought in each test performance.

Procedure 1. The first experiment was designed to compare finger oscillation under various tension loads with performance over an equivalent control period (legs prone and no experimentally induced tension). The *Ss*, the 10 undergraduate students, were divided into two groups: Group I performing under tension first and Group II under the control condition first. The tension condition was preceded by the basal relaxation previously described and consisted of maintaining scale pressures of 0, 10, 20, 40, and 80 lb. during rapid oscillation of the right index finger. The work-intervals were of 1-min. duration and were interspersed with 1-min. rest periods, during which *S* released his pressure on the levers. In the control period, the *Ss* went through the same 10-min. work–rest routine, but with legs prone. The average palmar skin-resistance was read for all work-periods. An interval of half an hour (or until *S* relaxed back to his original basal level) was maintained between the execution of the control and tension conditions.

Procedure 2. Because of the possible complication of cumulative fatigue and practice factors in the results, 5 of the *Ss* previously used did finger oscillation under tension conditions of Procedure 1 but with 5-min. intervals of rest between the successive 1-min. work-periods and with the tension-loads of 0, 10, 20, 40, and 80 lb. imposed in counterbalanced practice order rather than serially.

Procedure 3. This experiment utilized all 10 of the undergraduates and the task of finger oscillation. The conditions were the same as in Procedure 2 with the exception that every *S* first received extensive training in the maintenance of a given pressure for 1 min. and in 'relaxing away' its residual effects during the 5 min. between successive tests. Success in this matter was checked in terms of the degree to which the palmar resistance-level approximated that maintained during the basal activity of rest.

Procedure 4. This experiment differed from Procedures 2 and 3 only in the per-

[3] G. L. Freeman and R. M. Simpson, The effects of experimentally induced muscular tension upon palmar skin resistance, *J. Gen. Psychol.* (in press).

[4] The palmar resistance was always higher with the legs supported in this position than with them lying flat on the cot. The reason that the prone basal condition (which took an average of 20 min. to attain) could not be used as the starting point of the experiment is that we had no measurement (except palmar resistance) of the increment of tension from the basal to the zero condition.

formance utilized. This was mirror-drawing with a star pattern of standard size. The 10 *Ss* of previous experiments received several practice trials before beginning the experimental series.

Procedure 5. Several casual experiments were made with two graduate students, utilizing the habituated acts of eyelid winking, sensory discrimination and mental arithmetic as test performances, and a serial arrangement of the 5 tension loads.

Results

The effect of various tension-loads upon finger oscillation is compared in Fig. 1 with performance during an equivalent control-period. These curves are composited from the scores made by the 10 *Ss* under Procedure 1.[5] Solid lines indicate performance and broken lines show the accompanying palmar skin-resistance. In both cases the heavier lines denote the tension condition and the lighter lines denote the control. From the values plotted, there is established a presumption that tension-loading is reflected in the resistance-level and that increments facilitate finger-oscillation. The trend as shown in the composite curves is true for 7 out of the 10 individual curves, but since this is also the direction of practice-effects it was thought well to repeat the observations with the tension-loads in counterbalanced order. Furthermore, irregularities in the trend toward lowered resistance with increasing tension could be more effectively surveyed with a longer rest-interval imposed between tests.

Fig. 2. shows the influence of the graded scales of pressures upon finger-oscillation and palmar skin-resistance as developed in connection with Procedures 2 and 3. Solid lines indicate performance and dotted lines give the accompanying palmar resistance-level. It will be noted that the composite performance-curves for the 5 *Ss* in Procedure 2 does not show the same point of inflection from facilitative to inhibitory effects as does the composite curve for the 10 *Ss* in Procedure 3. This is to be expected, as the point of inflection differs from individual to individual and the occurrence of it in a composite curve depends upon the factors entering into the averages. The important thing is that in both experiments an increase in the number of finger-oscillations per minute occurs under increasing tension-loads until a certain point is reached after which there is recorded a decreasing work-output. This was true for 11 of the 15 individual curves, and of the 4 cases not showing inflection, 3 had increasingly effective performance under increasing tension-loads and only 1 gave no pronounced trend. The resistance-levels accompanying the various loads are more consistent in these experiments than in Procedure 1, and show the tendency reported in a previous paper for these measurements to reflect quantitative increments in muscular tension.[6]

On the whole, it may be said that the results of Procedures 1, 2, and 3 support the view that increments in tension are mainly facilitative for finger-oscillation; the optimal loading occurs late or not at all in the series of tensions employed. As to whether the optimal loading-point will be different for another type of task we refer to the results of Procedure 4, in which the effects of various tensions upon mirror-drawing was studied in the case of the 10 *Ss* used on the tests of finger-oscillation.

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[5] Any one wishing to treat these data by other methods may obtain mimeographed copies of the raw scores from the author.

[6] Freeman and Simpson, *op. cit.*

Composite scores are plotted in Fig. 3, with the solid line representing changes in performance and the dotted line the accompanying changes in palmar skin-resistance. It will be noted by comparison of Figs. 2 and 3, that the optimal loading-point occurs earlier for mirror-drawing than for the simpler task of finger-oscillation. This was shown very clearly in the case of 7 of the 10 *Ss*, the tension having little differential effect on the performance of the other 3.

The several experiments made under Procedure 5 tended to confirm the results of the first four procedures; that is, for the same *S* in otherwise identical conditions,

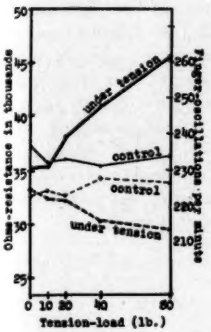

FIG. 1. RELATION OF TENSION-LOAD TO FINGER-OSCILLATION AND PALMAR SKIN-RESISTANCE (Solid lines = Av. no. finger-oscillations per minute; broken lines = Av. palmar skin-resistances.)

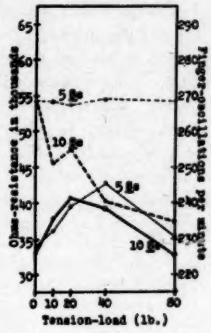

FIG. 2. RELATION OF THE DIFFERENT VARIABLES FOR THE TWO GROUPS OF *Ss* (Solid lines = Av. no. finger-oscillations per minute; broken lines = Av. palmar skin-resistances.)

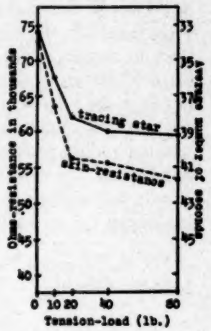

FIG. 3. RELATION TO TENSION-LOAD TO MIRROR-DRAWING AND PALMAR SKIN-RESISTANCE (Solid line = Av. no. sec. to trace star; broken line = Av. palmar skin-resistance.)

there tended to be differential tension-effects according to the performance under survey. Mental arithmetic was seriously disturbed by tension increments while sensory discriminations and reflex-responses were facilitated. Because the decrease in arithmetical performance under tension-load might possibly be explained by attentional shifts in a bifurcated task and also because not enough *Ss* were used to eliminate practice effects, the data are not formally presented in this paper.

DISCUSSION

The experiments herein reported indicate that a tension-load which is optimal for one type of activity may be detrimental to performance of a different character. In general, it is suggested that the more complex the performance, the earlier will the tension increment show inhibitory effects. Theoretically, then, it should be possible to describe for a given situation, a tension-load which will be maximally effective, with decreased performance occurring under loads which fall below or above it. The practical and experimental determination of such limits is beset by many pitfalls. Factors other than the amount of tension which must be taken into con-

sideration include (a) practice—the non-habituated act can utilize more tension to advantage than can the habituated act; (b) age-differences—since the problem of using tension to advantage involves learning to coördinate activity in the face of an intense excitation background, there is a presumption that adult performance will not be as adversely affected by great tension as will that of children; (c) major locus of tension in reference to the reaction-pattern—an equivalent amount of tension in one muscle-group may produce differential effects on two performances according as one task involves the tense muscle more than does the other; and (d) time-factors—if the tension load occurs in different temporal relation to two tasks, it is presumed that performance will be differentially affected. In our experiments we have used *Ss* of equivalent age and practice and have applied the different tension-loads to muscles which would seem to have no specific anatomical connection with the tasks under survey. Elsewhere we have discussed the interrelations of tension-factors in the support of performance.[7] We would add here a word of caution against an uncritical application of these results. *Ss* who were trained in tensing and relaxing various parts of the body provided the most valid evidence of optimal tension-loading. Cases in which the relation was not as apparent usually occurred in *Ss* who had difficulty in relaxing during the preëxperimental period and in maintaining during the tests minimal tensions in bodily parts other than the legs.

[7] G. L. Freeman, The optimal locus of 'anticipatory tension' in muscular work, *J. Exper. Psychol.*, 21, 1937, 554-564.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE

LXVIII. The Comparative Efficiency of Intensity, Perspective, and the Stereoscopic Factor in Producing the Perception of Depth

By M. F. Washburn and Constance Wright

This study undertook to reduce the problem of the perception of depth to its simplest terms by isolating the factors of intensity, perspective, and stereoscopy from all extraneous conditions. The objects whose relative distance away from the observer was to be judged were vertical lines on a white card. All the cards were looked at through a stereoscope. Each carried a left-hand pair of lines and a right-hand pair, even where the stereoscopic factor was absent and the lines in each pair were equidistant from each other.

There were 100 *O*s. The observations were made by artificial light. *O* sat before an ordinary stereoscope clamped to the edge of a table in a position identical for all *O*s except that its height was adjusted for different *O*s. *O* was allowed a little preliminary practice in fusing the two retinal fields.

The following instructions were then given:

In this card, as in every succeeding card, I wish you to tell me which of the two lines seems to be nearer you. One line may seem slightly nearer, decidedly nearer, or overwhelmingly nearer; or they may seem equidistant from you.

When the first card involving the stereoscopic factor was presented, *O* was warned that the fusion would be more difficult and brief, requiring special attention. As a general rule, when *O* had once fused one of the stereoscopic factor cards she had no further difficulty; for the few *O*s who found these cards troublesome they were presented later in the series.

The cards were as follows, the order being that of their presentation.

(1) No factor of depth present. On each half of the slide were two vertical lines as nearly as possible of equal blackness, both 5-cm. long, the distance between the members of each pair being 1 cm. The two figures were thus identical, and the four lines of equal length and blackness.

(2) The factor of 'intensity' alone present. Like (1) except that the right line of each pair was approximately twice as heavy as the left line of the pair. The factor of 'intensity' would make the combined right lines look nearer than the combined left lines.

(3) The factor of perspective absent; the factors of stereoscopy and 'intensity' opposed to each other. All four lines were of equal length. The left line of each pair was approximately twice as heavy as the right line: this would tend to make the fused left lines look nearer than the fused right lines. The distance between the lines of the left pair was 1 cm.; that between the lines of the right pair was

* Accepted for publication September 28, 1937. In the illness of Professor Washburn, her notes were transcribed by Dr. Polyxenie Kambouropoulou.

only 5 mm.: a condition which in the stereoscope tends to make the fused right lines look nearer than the fused left lines.

(4) The factor of 'intensity' absent; the factors of stereoscopy and perspective combined to make the left line look nearer. All the lines were as nearly as possible of equal blackness. The left lines of each pair were 5 cm. long; the right lines 4 cm. long; the top of the shorter line in each pair being 5 mm. below the top of the longer line and its bottom 5 mm. above the bottom of the longer line. The distance between the lines of the right pair was 1 cm., that between the lines of the left pair 5 mm.

(5) The factor of stereoscopy absent; the factors of perspective and 'intensity' combined to make the right line look nearer. The distances between the lines of the two pairs were equal (1 cm.); the right-hand lines were heavier than the left-hand lines; the right-hand lines were 5 cm. long and the left-hand lines 4 cm. long, the relative position of the short and long lines being as described in 4.

(6) The factor of perspective alone present. The lines were of equal blackness, the distances between the members of each pair were the same; the two left-hand lines were 5 cm. and the two right-hand lines 4 cm. long, the relative positions of short and long lines as in 4 and 5.

(7) The factor of perspective absent; the factors of 'intensity' and stereoscopy combined to make the right line look nearer. The distance between the lines of the right-hand pair was 5 mm.; that between the lines of the left-hand pair 1 cm.; the two right lines of each pair were heavier.

(8) The factor of 'intensity' absent; the factors of perspective and stereoscopy opposed. All four lines were of equal blackness. The right-hand lines of each pair, were longer than the left-hand lines in the same proportion as in slides 4, 5, and 6. The distance between the left-hand pair of lines was 5 mm.; that between the right-hand pair 1 cm.

(9) The factor of stereoscopy absent; the factors of 'intensity' and perspective opposed. The distances between the lines of the two pairs were the same. The right-hand lines of each pair were blacker than the left-hand lines; the left-hand lines of each pair were longer than the right-hand lines.

(10) The factor of stereoscopy opposed to both perspective and 'intensity.' The distance between the left-hand pair of lines was 5 mm.; that between the right-hand pair 1 cm.; this tended to make the left-hand line look nearer in the stereoscope. The right-hand lines of both pairs were both blacker and longer than the left-hand lines, the lengths being 5 and 4 cm. respectively and the relative positions as in slides 4, 5, and 6.

(11) The factor of perspective opposed to both stereoscopy and 'intensity.' The lines were drawn so that stereoscopy and 'intensity' would tend to make the left-hand line look nearer and perspective would tend to make the right-hand line look nearer.

(12) The factor of 'intensity' opposed to both stereoscopy and perspective. The lines were drawn so that stereoscopy and perspective would tend to make the left-hand line look nearer and intensity to make the right-hand line look nearer.

(13) The factors of stereoscopy, perspective, and 'intensity' combined to make the right line look nearer.

(14) The factor of stereoscopy alone present, tending to make the right-hand line look nearer.

The scoring of the factors was as follows: If the *O* saw the lines on a card as equally away from her, the factor present in that card and designed to work for a difference in depth perception was scored zero. If the *O* saw one line as slightly nearer, the factor responsible for this difference in depth perception was scored 1; if decidedly nearer, 2; if overwhelmingly nearer, 3. After *O* ran through the series once, she was given a few minutes of rest and then the series was presented in reverse order. As there were 100 *O*s the highest possible score that a factor or group of factors could get in a card was 600.

Results

(1) Score of victories of each factor acting alone against the absence of any factor (Cards 2, 6, and 14): Perspective, 198; stereoscopy, 195; 'intensity,' 112. Thus perspective is 1.77 times as efficient as 'intensity;' stereoscopy 1.74 times as efficient as 'intensity;' and perspective 1.0015 times as efficient as stereoscopy.

(2) Score of victories of one factor acting alone against one other factor (Cards 3, 8, and 9):

'Intensity' over perspective, 182; perspective over 'intensity,' 124. Thus intensity is 1.47 times as efficient as perspective.

'Intensity' over stereoscopy, 114; stereoscopy over 'intensity,' 191. Thus stereoscopy is 1.67 times as efficient as 'intensity.'

Perspective over stereoscopy, 115; stereoscopy over perspective, 225. Thus stereoscopy is 1.956 times as efficient as perspective.

(3) Contributions of each factor when two factors are acting together against the absence of any depth factor (Cards 4, 5, and 7).

Stereoscopy and 'intensity,' score of victories 311; 'intensity' acting alone, 112; contribution of stereoscopy, 199.

Stereoscopy and 'intensity,' 311; stereoscopy acting alone, 195; contribution of 'intensity,' 116.

Stereoscopy and perspective, 389; perspective acting alone, 198; contribution of stereoscopy, 191.

Stereoscopy and perspective, 389; stereoscopy alone, 195; contribution of perspective, 194.

'Intensity' and perspective, 272; 'intensity' alone, 112; contribution of perspective, 160.

'Intensity' and perspective, 272; perspective alone, 198; contribution of 'intensity,' 74.

(4) Contributions of each factor when two factors are acting against a third (Cards 10, 11, and 12):

Stereoscopy and 'intensity' over perspective, 253; stereoscopy over perspective, 225; contribution of 'intensity,' 28.

Stereoscopy and 'intensity' over perspective, 253; 'intensity' over perspective, 182; contribution of stereoscopy, 71.

Perspective and 'intensity' over stereoscopy, 232; 'intensity' over stereoscopy, 114; contribution of perspective, 118.

Perspective and 'intensity' over stereoscopy, 232; perspective over stereoscopy, 115; contribution of 'intensity,' 117.

Stereoscopy and perspective over 'intensity,' 318; perspective over 'intensity,' 124; contribution of stereoscopy, 194.

Stereoscopy and perspective over 'intensity,' 318; stereoscopy over 'intensity,' 191; contribution of perspective, 127.

(5) Score of victories for each factor acting alone against the two others (Cards 10, 11, and 12):

'Intensity' over stereoscopy and perspective, 68; perspective over stereoscopy and 'intensity,' 96; stereoscopy over perspective and 'intensity,' 148. In overcoming the other two factors stereoscopy is 2.18 times as efficient as 'intensity' and 1.54 times as efficient as perspective; perspective is 1.41 times as efficient as 'intensity.'

(6) Contributions of each factor when the three factors are acting together against the absence of any depth factor (Card 13):

Stereoscopy, 'intensity,' and perspective score of victories, 463; intensity and perspective against no depth factor, 272; contribution of stereoscopy, 191.
Stereoscopy, 'intensity,' and perspective score of victories, 463; stereoscopy and 'intensity' against no depth factor, 311; contribution of perspective, 152.
Stereoscopy, 'intensity,' and perspective score of victories, 463; stereoscopy and perspective against no depth factor, 389; contribution of 'intensity,' 74.

(7) Cumulative score of each factor acting alone, acting in opposition to other factors and acting in combination with other factors derived from the above scores:

'Intensity,' 885; perspective, 1284; stereoscopy, 1605. Stereoscopy is 1.81 times as efficient as 'intensity' and 1.25 times as efficient as perspective; perspective is 1.45 times as efficient as 'intensity.'

Conclusions

(1) 'Intensity' is the weakest of the three factors when each is acting alone against the absence of any depth factor. It is the weakest in its contribution when acting with one or with two other factors against the absence of any depth factor. In acting alone against one other factor 'intensity' is weak against stereoscopy but strong against perspective. In acting with one other factor against a third, the contribution of 'intensity' is weak in combination with stereoscopy and relatively strong in combination with perspective. In acting alone against the other two factors 'intensity' is the weakest of the three factors.

(2) Perspective is the strongest of the three factors when each is acting alone against the absence of any depth factor. It is medium in its contribution when acting with one or with two other factors against the absence of any depth factor; in its combination with stereoscopy against the absence of any depth factor it contributes somewhat more than stereoscopy. In acting alone against one other factor, perspective is the weakest of the three factors; it yields both to 'intensity' and to stereoscopy. This weakness of perspective against another factor in contrast to its strength when acting against the absence of any depth factor is probably due to the possibility of perceiving the longer vertical line as more distant than the shorter, if one perceives the lines as different in length. In acting with one other factor against a third the contribution of perspective in combination with 'intensity' against stereoscopy is practically equal to the contribution of 'intensity;' the contribution of perspective in combination with stereoscopy against 'intensity' is less than the contribution of stereoscopy. In acting alone against the other two factors perspective is weaker than stereoscopy and stronger than 'intensity.'

(3) Stereoscopy is almost, but not quite, as strong as perspective when each is acting alone against the absence of any depth factor. It is the strongest of the three factors in its contribution when acting with one or with two other factors against the

absence of any depth factor; in its combination with perspective against the absence of any depth factor it contributes somewhat less than perspective. In acting alone against one other factor, stereoscopy is stronger than perspective, stronger than 'intensity' and stronger than 'intensity' is over perspective. In acting with one other factor against a third, the contribution of stereoscopy is strong when stereoscopy and perspective are combined against 'intensity;' when stereoscopy and 'intensity' are combined against perspective, the contribution of stereoscopy is not as strong as in the previous combination but it is stronger than the contribution of 'intensity.' In acting alone against the other two factors stereoscopy is the strongest of the three factors.

(4) The efficiency of combinations of two factors is greatest when stereoscopy and perspective are combined, medium when stereoscopy and 'intensity' are combined and least when perspective and 'intensity' are combined.

(5) When the conditions of the experiment are considered as a whole, the most efficient factor in the perception of depth is stereoscopy, the next efficient perspective and the least efficient 'intensity.'

APPARATUS

AN APPARATUS FOR MEASURING CHANGES IN BODILY RESISTANCE

By Alvhh R. Lauer and Donald E. Anderson, Iowa State College

To fulfill the specific need for an instrument with which to measure changes in bodily resistance in a moving vehicle or under conditions of vibration, the following apparatus was designed. It was constructed so as to be rugged enough to stand jar and sensitive enough for delicate measurements. Darrow has developed a small reflexohmmeter for this work,[1] but at the time this apparatus was devised his paper was unpublished. Small and compact, Darrow's instrument is suited for demonstrational work and for measuring changes greater than 1000 ohms. Our apparatus is designed for research purposes as its characteristics allow for measurements of changes of bodily resistances as great as 66% varying from 4000 to 60,000 ohms. Changes as low as one tenth of 1% can be noted on the scale at all ranges and changes of 1% are easily recorded. The vernier measurements of changes in current are directly proportional to resistance changes in percentage and can therefore be read from the microammeter scale by calibration. Other derived measures may be used if desired.

Description. An ohmmeter is a galvanometer or other current measuring instrument having the scale or dial calibrated in ohms for measuring resistance of a circuit. The more common uses of ohmmeters are found in radio service work and various electrical installations. Accuracy greater than 5% is rarely necessary in such work and usually one or two scale ranges are sufficient. The ordinary ohmmeter circuit in commercial instruments is a simple series circuit consisting of meter, internal resistance, and the battery. Frequently in more expensive units, some means is provided for changes in battery voltage. For our purpose, however, it was necessary to have an instrument with a large number of ranges and so designed that it would give consistent accuracy.[2]

One of the objections to some simple types of ohmmeters is that no provision is made for changes in battery voltage as the ohmmeter is used from day to day. To compensate for the voltage decrease with age and use, a shunt (10, Fig. 1) is necessary. The range of this shunt is chosen to allow the ohmmeter to remain accurate until the battery voltage has dropped from 1.5 volts to about 1.29 volts for each cell.

Calibration. In measuring bodily resistance it is necessary to use an ohmmeter covering an approximate range of from 4000 to 60,000 ohms. Let us first consider the error involved in making measurements by means of a 0–1 milliampere meter. It is possible with the naked eye to read to the nearest 0.005 milliampere with certainty on the model 301 Weston meter and the percentage change in R_x per 0.005 m.a. at various positions on the scale may be determined. It is found by calibration

[1] C. W. Darrow, The reflexohmmeter (pocket type), *J. Gen. Psychol.*, 10, 1934, 238-239.

[2] Mathematical proof of the characteristics of the circuit will be furnished on request.

that the error of reading is at a minimum for values of current between 0.40 to 0.60. This represents a total change in resistance of about 55% and will be found sufficiently large for practically all such resistance measurements using a wet electrode at least 4 cm. in diam.

The ohmmeter used as part of the responsohmmeter is designed to be set for each of 6 different ranges to cover the extreme values needed. This is done by the use of different voltage E with corresponding changes in the value of R. The ohmmeter is

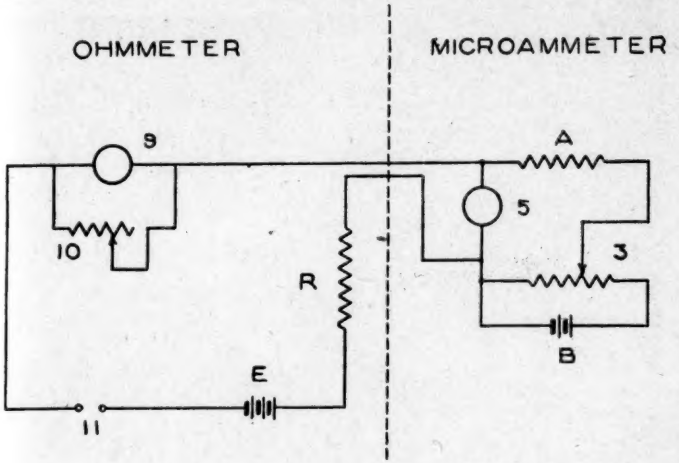

FIG. 1

FIG. 1. FUNDAMENTAL ELEMENTS OF CIRCUITS USED

Ohmmeter

E = Battery
R = Ohmmeter current limiting resistance
9 = Milliammeter
10 = Meter shunt
R_x (11) = Unknown resistance terminals

Microammeter

A = To provide high resistance between meter and battery circuit
B = Balancing battery
3 = Balancing control
5 = Microammeter

then calibrated by the use of standard resistance boxes having an accuracy of approximately 1%.

With the ohmmeter set up as described, it is possible to measure changes in resistance of about 2%. This is not of sufficient sensitivity, however, for the smaller changes in the resistance desired to be measured. By inserting a much more sensitive instrument in the circuit with the milliammeter and balancing out the main current it is possible to measure the increase or decrease in current from any given value. In this instrument a 0–30 microampere model 600 Weston meter is used.

The changes in current, as measured by the microammeter, are proportional to the percentage change in resistances of the body and a curve may be plotted between such current variation and the percentage change in resistance.

When the microammeter is introduced into the ohmmeter circuit and balanced to read 0 it does not affect the calibration of the ohmmeter and the initial reading of the absolute resistance is taken on the milliammeter scale with the microammeter completely balanced. A calibration curve showing the percentage change in resistance

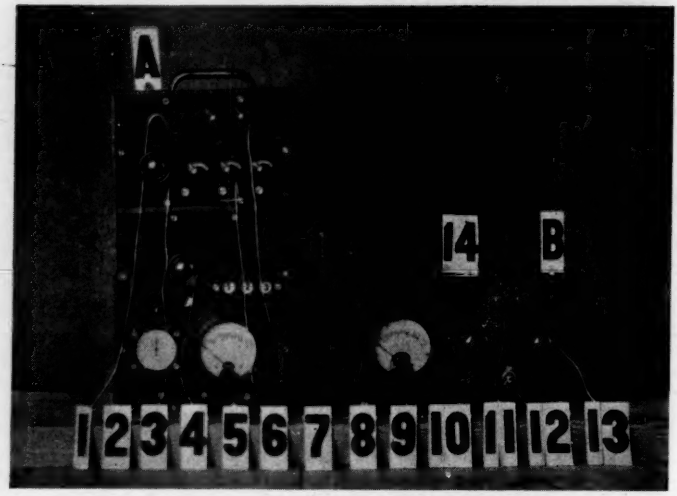

FIG. 2. SHOWING APPARATUS LAYOUT FOR OHMMETER

1 = Signal light
2 = Winding mechanism and stop watch
3 = Control rheostat
4 = Stimulus-dial
5 = Microammeter
6 = Three shunts to control range
7 = Battery switch
8 = Plug-in jack
9 = Milliammeter
10 = Rheostat for standardizing current
11 = Electrode connections for X
12 = Shorting switch
13 = Scale range dial
14 = Stimulus-signal button
A = Micro-ohmmeter
B = Ohmmeter

with the change in current is determined experimentally and thus corrections may be made for any change in conditions due to an unbalanced circuit.

The resistance of the microammeter which enters into the ohmmeter circuit when unbalanced will obviously have more effect on the lower scale ranges and towards the low resistance side of each range.

A change in resistance of about 0.4% per microampere can be measured. This represents the maximum sensitivity obtainable with this microammeter. It is not difficult to read to one-fourth of one microampere so that it is possible to detect changes as small as one-tenth of 1%. This is generally conceded to be ample for

most needs in measuring changes in bodily resistance. Arrangements are made, however, for shunting the instrument to different sensitivities in order to provide for wider percentage changes than could be handled at maximum sensitivity.

Additional features. In order to make photographic records of responses, either in the laboratory, or while in a movable vehicle, the features described below and shown in Fig. 2 were developed. Cabinet B contains the ohmmeter while A contains the micro-ohmmeter. The plug-in jack (8) connects the two units as shown in the drawing diagram, Fig. 2, cabinet B is placed in the seat beside the experimenter. The panel and electrical units of cabinet A are mounted on a 5-ft. board which rests across the tops of the seats. A 16-mm. ciné-kodak is mounted at a distance of 2 ft. of photograph the microammeter (5) and stop watch (2). The shunts (6) used are indicated by illumination back of the small white numbers on the panel. When the shunt is turned on the opaque black numeral records on film.

Knob (4) controls the stimulus-indicator lettered on a disk so that the release of the stimulus is accompanied by a flash bulb which records the letter in the same manner as that for the shunts described above. The stop watch may be wound from the panel and is used for timing the interval between stimuli.

The above equipment is thoroughly reliable and free from difficulty. The batteries last from 6 to 12 mo. and drops in voltage are adequately compensated by standardizing the current. It is also an excellent demonstrational instrument as the balancing operation is simple and the dial plainly visible. A direct comparison of subjects may be made by virtue of the percentage change principle. By use of a delicate fuse there is little or no danger of ruining the meters. The additional features described here are not needed for ordinary purposes.

A NERVE-ACTION POTENTIAL AMPLIFIER

By FRANKLIN HENRY, University of California

The development of new types of vacuum tubes, as well as the improvement of auxiliary apparatus used with them, has made it possible to considerably simplify the design of the electronic amplifier, an indispensable tool in modern studies of action potentials in nerve.

In order to be a more or less standard piece of laboratory equipment, such an apparatus should be self-contained and rugged, readily portable, and usable by experimenters who have had no specialized training in vacuum-tube technology. Batteries should be eliminated if possible, to avoid the need of the periodic checking and attention that their use entails. The sensitivity should be of the order of a few microvolts, and the amplifier should be powerful enough to operate an oscillograph. It should be noted, however, that power in excess of this amount will serve no useful purpose.

The amplifier being used by the writer is a four stage resistance-coupled arrangement that seems to meet the above requirements. It is completely operated from the alternating current line, uses metal tubes, is 11 x 17 x 9½ in. in size, and weighs about 25 lb. To use it, it is only necessary to attach a ground connection and electrodes, turn on the switch, and adjust a single control to obtain the desired amount

of voltage amplification. A milliammeter gives a quantitative measurement of nerve potential that is adequate for most purposes, while head-telephones serve qualitatively. With the input short-circuited, the background noise is apparently 3 or 4

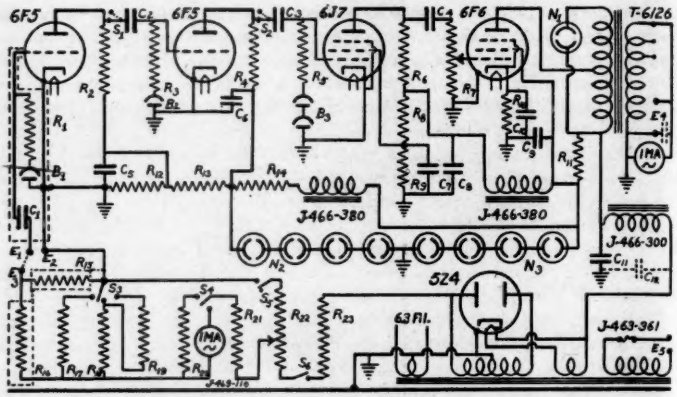

FIG. 1. AMPLIFIER CIRCUIT DIAGRAM

The two 2.5 volt filament windings on the power transformer (not indicated in the diagram) should be grounded at their mid-points. Amplification may be appreciably increased, with a slight loss of stability, if R_{12} is omitted. Regulator N_2 is operated below the ignition point; it is necessary to ignite it by inducing a surge by momentarily opening the A.C. line at E_5 after the amplifier has warmed up. For this reason, at least one of the bulbs of this regulator must be visible. If R_{22} will not adjust the calibrating meter to full scale, shunt R_{22} with a one or two megohm resistor.

C_1 to C_4=0.5 Mfd., 400 V. paper
C_5 to C_9 = 4.0 Mfd., 400 V. paper
C_{10} = 25 Mfd., 50 V. electrolytic
C_{11} = 8 Mfd., 500 V. paper
C_{12} = 1 to 3 Mfd., 600 V. electrolytic (see text)
B_1 to B_3 = Mallory bias cells
R_1, R_7 = 0.5 megohm, 1 watt
R_2 = 0.1. megohm, 1 watt
R_3, R_5 = 1.0 megohm, 1 watt
R_4, R_6 = 0.25 megohm, 1 watt
R_8 = 0.20 megohm, 1 watt
R_9 = 0.05 megohm, 1 watt
R_{10} = 500 ohms, 2 watt
R_{11} = 1,500 ohms, 25 watt (May be as high as 3,000 ohms if C_{12} is used.)
R_{12}, R_{13} = 0.25 megohm
R_{14} = 0.21 to 0.23 megohm
N_1 to N_3 = Neon bulbs (see text)
R_{15} = 1 ohm, 1% "Precision" wire wound, 1 watt
R_{16} = 10,000 ohm, 1% "Precision" wire wound, 1 watt
R_{17} = 10 ohm, 1% "Precision" wire wound, 1 watt
R_{18} = 101 ohm, 1% "Precision" wire wound, 1 watt
R_{19} = 1000 ohm, 1% "Precision" wire wound, 1 watt
R_{20} = 600 ohm metalized, 1 watt
R_{21} = 20,000 ohm metalized, 1 watt
R_{22} = 10,000 ohm wire wound, 1 watt
R_{23} = 0.150 megohm metalized, 1 watt

microvolts, since the meter shows a background level of less than 1/3 when the amplification control is so adjusted that a 10 microvolt 500 cycle input gives full scale deflection on the meter. This compares very favorably with more elaborate amplifiers using special low noise level vacuum tubes. With the amplification set at

a higher level, it is possible to just notice the introduction of 1 microvolt at the input.

Wiring diagrams for the amplifier,[1] and for a simple attenuator for calibration purposes, are given in Fig. 1. It will be noted that the output transformer is connected to a 3 watt 120 volt General Electric neon bulb (N_1) from which the metal base has been cut with a hack saw, permitting the removal of the series resistor originally built into it. This lamp has no effect unless for some reason the output is overloaded. When this occurs it flashes on and the alternating currents in the two parts of the transformer, which are 180° out of phase, cancel out. In this manner the output potential of the transformer is prevented from exceeding about 8 volts, regardless of the input to the amplifier. Limitation to this amount is sufficient to protect the meter from mechanical injury due to excessive current (meter injury is usually of this type). This has proved to be a very useful device, since in such a high-gain amplifier, excessive output voltages are encountered if the active input electrode happens to be pulled away from the preparation, or if it is touched by the experimenter while the amplification control is in an advanced position. The voltage-limiter also seems to have been effective in preventing the capacitor, transformer, and choke short-circuits that are occasionally apt to occur from the same causes.

Neon bulbs (with resistances removed) are also used as voltage regulators at N_2 and N_3, in order to reduce the effect of fluctuations in the alternating current supply line. Improved regulation can be secured by increasing the value of R_{11} and making up for the voltage loss by the addition of a few microfarads of capacitance C_{12} at the filter input. The circuit conditions shown proved adequate for the variations encountered in this particular installation.[2] Voltage regulation is secured by virtue of the fact that gas-discharge tubes do not obey Ohm's law, but tend to maintain a constant voltage drop across their terminals, hence most of the voltage variation appears across R_{11} where it only has a small effect on the amount of amplification.

Care has been taken to use only 'quantity production' parts that are easily and economically obtainable, and are preformed. For this reason, the construction of the amplifier is largely a matter of assembly. A few small holes must be drilled, and the first-stage socket should be mounted on flat springs to prevent microphonic disturbances. All parts of the first stage input circuit are shielded from the rest of the amplifier. This can easily be done with a few pieces of tin, using the corners of the chassis as part of the shield. The first two tubes are provided with grid caps. Shielded wire is used for input and grid leads. Output leads should also be shielded and the meter should be of the metal-cased type in order to prevent feed-back that would injure the nerve preparation. The use of a shielded room is unnecessary, but the preparation should be within, and insulated from, a grounded copper-mesh box. One side of this may be open or partially so. Input and output connection plugs are of the shielded type.

Resistors and condensers may be mounted on insulated connection strips that are

[1] In some cases, the parts are identified by a manufacturer's catalog number—such numbers preceded by J refer to Jefferson Electric Co., (Bellwood, Ill.) catalog 362R, and those preceded by T to Thordarson Electric Mfg. Co., (Chicago, Ill.) catalog 345. The bias cells are of the Mallory type which do not require periodic replacement.

[2] C. W. Brown and the writer are using the apparatus for quantitative measurements of eighth nerve potentials.

commercially available. Soldering must be done with rosin flux to prevent noise-producing leakage. The output transformer is mounted in a corner of the chassis, with a tin shield around the exposed side. The other transformers and chokes are mounted on the outside of the chassis. Chokes should be experimentally oriented so that hum pick-up from the 60 cycle transformer is at a minimum. All filament leads are tightly twisted together to prevent hum. The filament leads to the first tube should be reversed, as it is usually found that one of the possible polarities gives less hum than the other. About a third of the commercially available tubes used in the first stage have a high hum level; for this reason a number of tubes should be tested in this position, allowing them to become thoroughly heated before the test measurements are made. If the above precautions are observed, the hum level may be kept as low as the electronic noise level. A 0.1 or 0.5 Mfd. condenser connected from one side or the other of the 60 cycle line to ground sometimes reduces hum, and in some installations reversing the attachment plug helps. It is of course necessary to keep all unshielded 60 cycle wires and lights well away from the input circuits and preparation, and nearby metal objects should be grounded if they are close to the 60 cycle line. Socket shells and reflectors, particularly, are likely to require grounding. It is most economical of effort to use the progressive elimination method if trouble is experienced from excessive 60 cycle pickup.

Since the frequency response curve of an amplifier of this type is quite flat, calibration may be carried out with the 60 cycle current through the use of the attenuating network (R_{16} to R_{28} inclusive). The output meter is disconnected and inserted into the attenuator, S_6 is turned on, and R_{28} is adjusted for full scale deflection; then the switch S_4 is thrown to the right, the meter is replaced in the amplifier, and the calibrating link between E_1 and E_2 is closed. If two meters are available, one may be left in the attenuator during calibration in order to secure intermediate calibration points. An audio-frequency oscillator can be used instead of the 60 cycle current, making possible calibration at other frequencies. If this is done, S_6 is opened and the oscillator is connected between this point and ground. If the current flow is insufficient, the link at S_5 should be opened—at all other times it is closed.

A three position switch is installed at S_3 to give calibration ranges of 1, 10 or 100 microvolts when it is thrown to R_{17}, R_{18}, or R_{19}. The accuracy of the copper-oxide rectifier meter is about 5%. The calibration may be depended on to about the same extent except at the 1 microvolt level.

The utility of this apparatus is not limited to nerve-action potential studies. With the addition of an oscillograph it can be used as an electro-cardiograph; the attachment of a crystal microphone now commercially available for the purpose converts it into a stethophone. With slight alteration, it can be converted into a cardiometer,[8] in which the action potential from the heart beat operates an electro-mechanical counter. Relative sound intensities can be measured—for example the writer has used a similar amplifier for determining the distribution of sound in a maze, and in an auditory discrimination apparatus used with laboratory animals. For most purposes, it is unnecessary to use an expensive microphone, as a head-telephone works very well as a sound pick-up if a shielded cord is used and the magnet is grounded. Where electro-magnetic devices generate the sound, it is better to use a

[8] F. Henry, An electric cardiometer, *Science*, 86, 1937, 229-230.

crystal headphone to prevent error from magnetic coupling. Usually the sound has an intensity great enough to permit the elimination of one or two amplifier stages by inserting the pick-up at S_1 or S_2. This should always be done when possible to prevent overload distortion.

Both second and third harmonic distortion are present to the extent of several percentages, but this is no disadvantage in the majority of action potential experiments. In case a low value of harmonic distortion is required, recourse may be had to filters, or if this is not possible a phase-inverter may be installed and the last two stages changed to the 'push-pull' type of circuit, as in the Garceau and Davis amplifier.[4]

Electro-encephalograms cannot be made with the circuit illustrated because of the extremely low frequency involved. Experiments in progress indicate that if the fixed-bias arrangement used in the cardiometer is employed, the limiting factor is the output coupling device. An oscillograph tube has been used as an output indicator for low frequencies with fair success, having first replaced the output transformer shown in the diagram with a type J467-428 driver transformer used in inverted fashion with from 10 to 30 Mfd. of capacity shunted across the output leads as indicated by the dotted lines at E_4. This modification should make encephalograms possible.

In conclusion, it may be stated that the apparatus has the following desirable features. It is moderate in cost and constructed from commercially available parts, the design is such that it can be operated indefinitely by a biological scientist without the assistance of a specially trained technician, and it is adapted to several laboratory uses.

APPARATUS FOR THE STUDY AND TREATMENT OF ENURESIS

By O. H. Mowrer, Yale University

In a more extensive report, which will appear elsewhere,[1] a method for the study and treatment of nocturnal enuresis has been described which has given highly satisfactory therapeutic results and which seems to be relatively independent of suggestion and the other intangible personal influences which appear to play so prominent a rôle in determining the success or failure of many other methods. The purpose of the present communication is to describe in somewhat greater detail than seemed appropriate in the study cited the exact specifications of the apparatus essential to the application of this type of treatment.

The function of this apparatus is simply to provide an automatic means of waking the child *immediately upon the commencement of urination* instead of allowing him to complete this act and sleep placidly on until morning or of depending upon a regular waking schedule which may or may not prove effective in establishing the

[4] E. L. Garceau and H. Davis, An amplifier, recording system, and stimulating devices for the study of cerebral action currents, *Amer. J. Physiol.*, 107, 1934, 305-310.

[1] O. H. Mowrer and W. M. Mowrer, A method for the study and treatment of enuresis, (in press).

desired dry habit. This end is accomplished by requiring the child to sleep on a specially designed type of pad which, when moistened by even a few drops of urine, completes an electrical circuit (the current in which is extremely weak and, moreover, cannot possibly come into contact with the child). The completion of this circuit through the pad activates a small, highly sensitive relay which in turn causes an electric bell (of adjustable loudness) to ring. The ringing of this bell almost invariably inhibits further urination; and if at first it does not also wake the child, it soon acquires the capacity to do so providing that the attending adult makes a practice of arousing the child while the bell is still ringing.[2] The cure is effected when, as a result of the repetition of this sequence of events, the waking response, instead of occurring to the bell, becomes conditioned to the stimulation incident to bladder distension and occurs in advance of the actual onset of urination.

The type of pad on which the child sleeps is made of two pieces of No. 16 mesh bronze screening, 24 x 32 in., which are separated by two thicknesses of coarse, absorbant cotton (bedspread) fabric and which are covered on the top side by another thickness of this same material. (In order to prevent the cut edges of the screening from fraying, it is necessary to apply to them a coating of 'Soldine' or some other good chemical 'solder.') When properly quilted together, this combination of cloth and wire is durable, light in weight, and not uncomfortable for the child to lie upon. Electrical contact is made with each of the two pieces of screening in the pad by means of a few feet of double lamp cord, the opposite end of which terminates in an ordinary telephone plug. Experience has shown that the ends of the individual wires which go to the two screens should be connected (by soldering) to the screens a few inches' distance inward from their edges, the one on the top and the other on the bottom side of the pad, and the wires whipped fast to the pad. Otherwise, if the wires are attached to the edges or corners of the screens, they soon break off as a result of the twisting and bending that is unavoidable in the handling of the pad. If, however, they are attached as indicated, they will remain intact throughout the life of the pad, which is from a year and a half to two years of continuous use.

When a pad has been wet, it should be immediately dried by being placed at a safe distance above an electric lamp. A light frame for supporting the pad while drying can be easily constructed. With prompt drying and otherwise reasonable care, pads of this type can be used for a surprisingly long time without becoming offensive; when washing does become necessary, it can be effectively carried out by immersing the pads in a solution of warm water, soap, and ammonia and allowing them to dry in the sunshine or over one or two electric lamps.

Of the remainder of the apparatus the relay is the only part which is critical. After considerable experimentation, the writer has found that an instrument with an internal resistance of 1000 ohms is most satisfactory.[3] A relay with lower resistance requires too much current to operate it, which means that an unnecessarily large area of the pad has to be wet before the relay will be activated. On the other hand, a relay with significantly higher resistance is too sensitive and may be activated if the

[2] In the early stages of the development of this method of treating enuresis, a specially constructed bed was used, one side of which could be caused to drop an adjustable amount if the bell alone did not suffice to produce waking; the simpler arrangement just described has, however, given entirely satisfactory results and makes the method more generally applicable.

[3] Type ER12, manufactured by Hugh H. Eby, Inc., Philadelphia, Pa.

pad becomes slightly damp due to humid atmospheric conditions or perspiration from the child. The electromagnet of the relay should be connected in series with a 22.5 volt radio "B" battery and a telephone jack, appropriate for receiving the plug attached to the pad; when the pad is wet this circuit is thus completed and the relay activated. The "B" battery should have a number of taps so that when it is fresh 9 or 10 volts can be used; after several months of usage, higher steps can be employed in order to maintain approximately the same operating voltage.

The contacts which come together when the relay is activated should be connected in series with two 6-in. dry cells, an electric bell (common annunciator type), and a 25 ohm variable resistance (radio filament control rheostat), the latter providing the means of controlling the loudness of the bell. The relay, bell, rheostat, and batteries can all be compactly mounted inside a small metal tool box (6 x 6 x 14 in.), and once the wiring is completed and a suitable setting of the rheostat obtained, the box can be locked and need not be opened for some months.[4] The telephone jack into which the plugs attaching to the pads are inserted is, of course, mounted on the wall of the box in such a way as to be accessible from the outside. After a pad is wet and the relay activated, all that is necessary in order to turn off the relay and thus terminate the ringing of the bell is to remove the telephone plug from the jack. A dry pad is then placed upon the child's bed (the child having been sent to the toilet in the meantime) and the attached plug reinserted into the jack; the apparatus is then again ready for use.

Despite examination of several hundred articles dealing with the etiology and treatment of nocturnal enuresis, the writer has been unable to find any published record of the present method of treatment having previously been employed. On May 4, 1936, the following appeared in the *Baltimore Sun* as a legend beneath an Associated Press photograph:

> Svordlovsk, U.S.S.R. Russian science has just announced this gift to Soviet motherhood—a light which flashes when baby needs changing. Wires from batteries are attached to strips of tinfoil in a special packet beneath the infant. Cloth sandwiched between the tinfoil becomes a conductor when dampened and, presto, the light goes on. The system is already in use in a hospital here.

There is here no indication that advantage was taken of this arrangement in order to treat enuresis, and the writer has been unable to obtain any further information from other sources. The apparatus for the study and treatment of enuresis described in the present communication has been in continuous use at the Children's Center in New Haven since November, 1935.

In addition to the value of this type of apparatus for the treatment of enuresis, it has proved useful as a method of studying this phenomenon somewhat more closely than has otherwise been possible. For example, it makes available an accurate record of the time and frequency of wetting, and since the child is awakened so promptly after the onset of urination, it is relatively easy also to elicit an account of the dreams or half-waking fantasies which have just preceded this act. For a fuller description of these and related findings, as well as of the therapeutic application and success of this method, the reader is referred to the more detailed report already cited.

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[4] The sound of the bell is changed somewhat as a result of the box being closed, but its loudness is not significantly diminished.

APPARATUS NOTES

The Reliability of Commercial Alternating Current for Operating Reaction-Time Chronoscopes

Any laboratory technician may easily check the cycle reliability of commercial current by means of the following simple apparatus. A good grade of tuning fork, with an adjustable load on the tines, is set at approximately 60.5 v.p.s. and mounted above a 2-milliampere glow lamp of the neon variety. Such lamps may now be secured from any Westinghouse lamp shop at low cost. When such a tuning fork is set into vibration, and watched with the glow lamp behind, an

TABLE I

Cycle Frequency Readings Taken from Commercial Current
(Lehigh University, September 11, 1937, 8:15 A.M. to 5:00 P.M., Eastern Daylight Saving Time.)

Time	Freq.	Time	Freq.	Time	Freq.	Time	Freq.	Time	Freq.
8:15	59.99	10:00	60.07	11:45	59.90	1:30	60.02	3:15	60.04
8:16	59.96	10:01	60.05	11:46	59.94	1:31	60.00	3:16	60.02
8:17	59.96	10:02	59.96	11:47	59.92	1:32	59.98	3:17	59.99
8:30	60.08	10:15	60.10	12:00	60.03	1:45	59.92	3:30	60.03
8:31	60.06	10:16	60.04	12:01	60.02	1:46	59.93	3:31	60.04
8:32	60.06	10:17	60.06	12:02	60.04	1:47	59.94	3:32	60.05
8:45	59.98	10:30	59.98	12:15	60.01	2:00	59.93	3:45	60.04
8:46	60.03	10:31	60.05	12:16	60.00	2:01	59.91	3:46	60.03
8:47	60.00	10:32	59.99	12:17	59.95	2:02	59.94	3:47	60.05
9:00	59.99	10:45	60.05	12:30	59.84	2:15	59.96	4:00	59.96
9:01	59.96	10:46	59.99	12:31	59.86	2:16	59.93	4:01	60.00
9:02	59.98	10:47	59.97	12:32	59.88	2:17	59.96	4:02	59.95
9:15	60.02	11:00	60.04	12:45	59.97	2:30	59.98	4:15	59.88
9:16	60.03	11:00	60.01	12:46	60.02	2:31	59.95	4:16	59.96
9:17	60.05	11:02	59.98	12:47	60.00	2:32	59.91	4:17	59.93
9:30	59.98	11:15	60.03	1:00	60.01	2:45	59.96	4:30	60.15
9:31	59.98	11:16	59.99	1:01	60.02	2:46	59.96	4:31	60.11
9:32	59.97	11:17	59.97	1:02	60.03	2:47	59.97	4:32	60.13
9:45	59.99	11:30	60.00	1:15	60.04	3:00	59.98	4:45	60.06
9:46	59.98	11:31	59.97	1:16	60.08	3:01	59.99	4:46	60.05
9:47	59.98	11:32	59.98	1:17	60.08	3:02	59.91	4:47	60.04

oscillating shadow will be seen by the sides of the vibrating tines. This shadow is the interference beat between the two frequencies and can be used as a reliable index for any variations in the commercial current frequency operating the glow lamp. The tuning fork frequently may be regarded as a constant if the temperature and atmospheric conditions are kept reasonably constant. Obviously, if the fork vibrates at 60.5 and the lamp at 60.0, the beat will be the difference between the two, or one full wave in 2 sec. Now, if the commercial current frequency should increase to as much as 60.25 per sec., the beat wave will change to one full wave in 4

sec., or one half the previous frequency. It is obvious that a small change in the lamp frequency produces a huge change in the frequency of the interference beat, so great that it is very easily measurable with a stop watch.

The test is made as follows: The stopwatch is started at the moment an interference wave reaches a crest. At exactly the same relative position of the wave, 10 beats later, the watch is stopped. The following formula now applies: $F_t - B/T = F_c$, wherein F_t is the frequency of the tuning fork, B is the number of interference beats counted, T is the time in seconds read from the stop watch, and F_c is the desired frequency of the commercial current.

The accompanying table shows a sampling of frequencies taken on September 11, 1937, and is very similar to cycle-chart records we have secured at previous times during the past two years.[1] This will show, therefore, a typical expectancy of the reliability of commercial current for the operation of synchronous chronoscopes, used in reaction-time experiments.

In general it is well to remember the conditions under which such current is produced. It is very seldom that appreciable variations will occur in a duration of one second. Nearly all alternating current now comes from generators with heavy armatures driven by smooth running, but heavy turbines. Further, large numbers of independent power producers synchronize their lines, one organization with another, and any generator cannot go faster or slower than the several thousand other generators synchronized with it. Nearly all the power producers in the New England and eastern section of the United States are thus tied together. Another large synchronized organization exists in the middle west, and still another on the west coast. If a local producer is not synchronized with a chain, then his cycles will probably be thoroughly unreliable. Allowances must be made for disturbances in local thunder storms.

As the table shows, even in a large synchronized system, the worst surges take place during the noon hour and between 4:00 and 4:30 P.M. This is due to the heavy shifts in industrial current demand.

For purposes of estimating the importance of the variations in the table, it may be stated that 0.06 cycle is exactly equal to one millisecond, in a duration of one second, for the sixty-cycle current.

Lehigh University ADELBERT FORD

A FILM OF THE NERVOUS SYSTEM

The talking, moving picture produced by Gerard in coöperation with the Department of Anatomy of the University of Chicago, gives a graphic introduction to the structure and function of the cerebrospinal nervous system.[2] For purposes of description, the contents of the film may be divided into four major parts: (1) the anatomy of the central nervous system; (2) the structure of the reflex arc; (3) the nature of the nervous impulse; and (4) the electrical recording of nervous impulses.

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[1] For an earlier calibration of the commercial current see S. W. Fernberger, The accuracy of phase cycles in a commercial electric current, this JOURNAL, 44, 1932, 582 f.

[2] R. W. Gerard, The nervous system (A film), *Erpi Picture Consultants*, 1937, 400 ft., with sound.

Brief illustrations of some typical reactions of the sensitive plant, amoeba, frog, cat and man preface a comparative study of the central nervous system. By using models, the structural similarities and the relative volumic differences of the brains of the frog, cat and human are clearly delineated. The point is made that an unusually small cerebrum in man is associated with idiocy (microcephalism).

The concept of the reflex arc is developed with animated diagrams which show the ramifications of peripheral neurons and their connections with internuncial fibers within the spinal cord. The effects of transecting the dorsal and ventral roots are exhibited in a cat with anesthesia and paralysis of its rear limbs.

Nervous impulses are diagrammatically represented as collars of local excitation spreading along separate fibers. The electrical potential of each impulse is shown to be dependent upon the local condition of the nerve fiber rather than upon the strength of the stimulus. The temporary depolarization of the excited fiber is lucidly illustrated by the technique of animation. A short sequence of Lillie's iron wire analogue lends concreteness to the above diagrammatic illustrations.

By employing an oscillograph and loud speaker the nervous impulses from a cat's paw are made visible and audible. Similarly, the waves from a resting cat's cerebrum and the faster rhythm of the human brain are vividly demonstrated. The film closes with a brief review of the rôle of nervous impulses in behavior.

A few minor criticisms of the film may be made. In the first place, the impression is given that there is a somewhat strict localization of function in the nervous system. Quoting from the narrative:

> But note further that the frog's nervous system has just the same parts as man's, though differing in relative size. And this must be so, since each part of the nervous system has a separate duty to perform, and these duties are fundamentally alike from frog to physiologist. For no other organ is this true. One would hardly miss one lung or half the liver.

In the light of the results reported by Dandy, Brickner, and Lashley, among others, it is doubtful whether the loss of one hemisphere is so serious as the above quotation implies. Secondly, the depiction of nervous impulses as collars or bulges coursing along a nerve fiber may prove somewhat misleading, especially to the elementary student, since the illustration gives the impression that the impulse is a physical rather than an electro-chemical change of the fiber. Thirdly, the autonomic division of the nervous system is disregarded. Undoubtedly, this omission is attributable to the complexity and importance of the autonomic division, which, even for an elementary delineation, would probably require a film as long as the present one.

The merits of the film, however, far outweigh those criticisms. The photography, sound, and organization are splendid. Sufficient practical illustrations are included to maintain a high level of interest. The topics of excitation, nervous conduction, refractory phase and the electrical registering of impulses which usually are difficult for the beginning student to grasp in the abstract, are presented in simple, concrete form. In large classes where demonstrations are difficult to arrange, this film will satisfy an urgent need. Finally, it should be welcomed by departments lacking the expensive demonstrational equipment of the sort used in some of the sequences.

University of Oregon L. F. Beck

NOTES AND DISCUSSIONS

Organic Amnesia and Relearning

In certain simple sensory discrimination problems, it has been found that although the discrimination can be formed in the absence of the corresponding cortical sensory area, complete destruction of that area after learning produces a loss in retention. With further training, however, the discrimination habit can be reëstablished. This has been shown to be the case for brightness discrimination in rats by Lashley,[1] and in dogs by Marquis;[2] and for auditory discrimination in rats by Wiley,[3] and Pennington.[4] Thus while the cortical sensory area is not indispensable for learning, it is somehow involved in the habit in the intact animal.

Two divergent hypotheses have been advanced in explanation of these somewhat anomalous results. The first, proposed by Herrick[5] and Brown,[6] suggests that some diencephalic or thalamic structure is essential for the habit, and the cortical sensory area exerts a specific facilitating effect upon its activity. The loss in retention of the discrimination problem after cortical injury is explained as being the result of the removal of this normal facilitation. The second, advanced by Lashley,[7] Ghiselli,[8] and Ghiselli and Brown,[9] suggests that there is an equivalence of function between the cortical sensory areas and the mesencephalon. In the intact animal, learning is accomplished through cortical activity. Ablation of cortical centers after learning destroys the engram and produces a loss in retention. With further training the mesencephalic centers vicariously assume the function in the reëstablishment of the habit.

Both of these hypotheses have one characteristic in common; namely, that the engram is localized either in cortical, diencephalic, or mesencephalic centers. The experimental facts, however, may be explained on another basis. It might be that metencephalic or even lower structures have as their function the retention of the engram. If this were the case, the higher centers would function as dynamic relay

[1] K. S. Lashley, The mechanism of vision: XII. Nervous structures concerned in the acquisition and retention of habits based on reactions to light, *Comp. Psychol. Monog.*, 11, 1935, 43-79.

[2] D. G. Marquis, Effects of removal of the visual cortex in mammals with observations on the retention of light discrimination in dogs, *Proc. Ass. Res. Nerv. & Ment. Dis.*, 13, 1934, 558-592.

[3] L. E. Wiley, The function of the brain in audition, *J. Comp. Neur.*, 54, 1932, 105-135; A further investigation of auditory cerebral mechanisms, *ibid.*, 66, 1937, 327-331.

[4] L. A. Pennington, The function of the brain in auditory localization: II. The effect of the cortical operation upon original learning, *ibid.*, 66, 1937, 415-442; The function of the brain in auditory localization: III. Postoperative solution of an auditory spacial problem, *ibid.*, 67, 1937, 33-48.

[5] C. J. Herrick, *Brains of Rats and Men*, 1926.

[6] C. W. Brown, Subcortical mechanisms in learning: I. The functional significance of subcortical nuclei in certain simple learning tasks, with a description of a program for further experimental work, *Psychol. Rev.*, 42, 1935, 307-334.

[7] K. S. Lashley, *op. cit.*

[8] E. E. Ghiselli, The superior colliculus in vision, *J. Comp. Neur.*, 67, 1937, 451-467.

[9] E. E. Ghiselli and C. W. Brown, Subcortical mechanisms in learning: III. Brightness discrimination, *op. cit.* (in press).

paths, discharging into lower centers in rhythms and patterns characteristic both of themselves and of the sensory impulses reaching them. Due to structural and functional relationships in the central nervous system, the cortical centers are dominant over the diencephalic and mesencephalic centers. Thus when the cortex is discharging into the metencephalon and lower centers, the diencephalon and mesencephalon are prevented from doing so. In learning, then, the characteristic cortical patterns and rhythms of discharge are those on the basis of which the engram is formed. If now the influence of the cortex be removed by surgical interference, these patterns are no longer available, and consequently amnesia results. At the same time the dominance exercised by the cortex over the diencephalic and mesencephalic centers is removed and therefore the latter are free to discharge their characteristic rhythms and patterns to the lower centers. Since these patterns are different, for a given stimulating situation, from those of the cortex, the old differential response cannot be rearoused by them. Further training, however, brings about a new learning process on the basis of these newly released patterns, and thus the habit is reëstablished.

Two additional bits of evidence lend the hypothesis presented above a certain degree of plausability. Prosser and Hunter,[10] and Culler[11] have obtained results which suggest that learning may occur even in spinal animals. It would seem, therefore, that responses mediated by even the lowest central nervous structures are modifiable.

The results from studies on pattern vision are readily expainable in terms of the present hypothesis. It has been found by Lashley,[12] and Lashley and Frank[13] that rats lacking the visual cortex are unable to establish or retain discriminations involving a differential response to visual patterns. This phenomenon could easily be interpreted as being due to the relatively simple structure and functional processes of the mesencephalic visual centers. The cortex is the structure *par excellence* for the translation of sensory patterns into neural rhythms for discharge into lower centers. The mesencephalic structures, on the other hand, due to their more primitive organization, are functionally incapable of translating such sensory patterns into differential neural rhythms.

The purpose of this note is to point out this third possible explanation of the experimental findings, which has thus far been overlooked.

Cornell University

F. Nowell Jones
Edwin E. Ghiselli

The Psychological Background of the Doctoral Candidate

Men who recall with distinctness the labors of their doctoral years will understand those periods of anxious depression which settle upon the graduate student when he seeks a general orientation for his doctoral dissertation and while he awaits

[10] C. L. Prosser and W. S. Hunter, The extinction of startle responses and spinal reflexes in the white rat, *Amer. J. Physiol.*, 117, 1936, 609-618.

[11] E. Culler, Observations on the spinal dog, Reported at the 45th annual meeting of the American Psychological Association (Minneapolis, 1937).

[12] K. S. Lashley, The mechanism of vision: IV. The cerebral areas necessary for pattern vision in the rat, *J. Comp. Neur.*, 53, 1931, 419-478.

[13] K. S. Lashley and M. Frank, The mechanism of vision: X. Postoperative disturbances of habits based on detail vision in the rat after lesions in the visual areas, *J. Comp. Psychol.*, 17, 1934, 355-380.

qualifying and final examinations. The depression is commonly due to a lack of general perspective as well as to the uncertain requirements of an examining committee. The candidate sighs "The little done, the undone vast," not knowing quite how to organize his efforts or to predict the range of the forthcoming ordeal.

Anticipating these periods of anxious depression, we have posted in the Research Laboratory at Cornell the following list of topics. The topics are to be taken rather as a general guide than as an exhaustive catalog of subjects. The list is reproduced here, not because the specific problems set possess special excellence or relevance but rather in the belief that this sort of guidance and sampling is of great use to most candidates for the doctoral degree, both enlarging and defining for them the field which they are expected to command with knowledge, skill, and wisdom. The posted sheet bears the heading "Topics and Problems in Current Psychology; Daily Exercises for the Doctoral Candidate."

The range and limits of psychology as understood by me, by psychologists at large, by the physician, psychiatrist, biometrist, educator, press, popular magazine writer, psychoanalyst, layman, neurologist, mental tester, sociologist, philosopher and anthropologist.

A critical appraisement of standard methods used by psychologists in and out of psychology.

A perspective survey of all the physiological and all the psychological operations of the living organism (man and other animals).

Compare that which psychologists and that which psychologies have done for related sciences and for the applied arts of business, medicine, law, education, etc.

Make (i) *a logical and* (ii) *a working distinction* between psychologist and neurologist. To illustrate the distinction, use the facts and methods of electrical potential in nerve, muscle and brain.

What subjects in general physiology should the psychologist know at first hand?

Estimate your own resources (in historical perspective, training, command of the researches in hand, knowledge of methods, etc.) for an independent study of the 'higher mental processes.' (Criticize each word of this ambiguous phrase.)

Deal critically with the various arts of observation and report in psychology, including the functions and the training of experimenter, observer, and subject.

Examine objectively and be prepared to defend your own general position in psychology. Search out the historical and biographical reasons for the views which you espouse and the methods which you prefer.

Name eight-to-twelve events in the history of psychology which you regard as of primary importance. Justify your choice.

Be clear upon the fundamental difference between psychotechnology and the arts of practice; between applied psychology and psychology applied.

Relate in a coherent way to each other and to a general psychology the following *special psychologies;* social, comparative and genetic psychologies; the psychology of disorder and deficiency and the study of psychological varieties. See whether you can derive them—one and all—from *a single set of basal psychological principles.*

Distinguish (i) acting and muscular responses, (ii) emoving and visceral-autonomic excesses, (iii) drives and psychological antecedents, (iv) physiological homeostasis and psychological government, (v) language and vocabulary, (vi) language and laryngeal movements, and (vii) bodily resources and dynamic mental mechanisms.

What answer would you return to the following query received from a collegiate senior?

"I have completed fundamental courses in psychology. What books, monographs, and articles should I read and assimilate to give me a fair, firsthand command of this subject in my graduate study? Please group the suggested works in a helpful way."

Twenty-odd varieties of psychology were expounded and defended in the volume *Psychologies of 1930*. Which of these lay a general foundation for psychology at large? What are the others? Which are protestant varieties? Which are importations from other subjects? Which are new subdivisions of old fields? Which lay claim to new territory? How many of them are to be amalgamated into your own working psychology?

Cornell University MADISON BENTLEY

THE FORTY-FIFTH ANNUAL MEETING OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

The Forty-Fifth Annual Meeting of the American Psychological Association was held at the University of Minnesota, Minneapolis, Minnesota, on Wednesday, Thursday, Friday, and Saturday, September 1, 2, 3, and 4, 1937. A new attendance record was established, the total being 854 persons, classified as follows: 163 Members, 355 Associate Members, 30 newly elected Associates, and 306 persons not affiliated with the Association.

At the Annual Business Meeting of the Association, which was held on Thursday evening, 3 Members were elected, 27 Associates were transferred to the status of Member, and 233 Associate Members were elected. The By-Laws were amended to provide for a third class of membership, to wit, Honorary Member. Members of the Association for at least twenty years who have reached the age of seventy may request this status. The invitation from Ohio State University to hold the 46th Annual Meeting at Columbus, Ohio, was accepted and the following dates were fixed: Wednesday through Saturday, September 7, 8, 9, and 10, 1938. Harold E. Burtt was appointed a member of the Executive Committee for 1937-1938, and Edmund S. Conklin, Edward B. Greene, and the Secretary were elected as the Program Committee for 1938. Invitations for 1939 and 1940 were also accepted as follows: Stanford University and University of California, Wednesday through Saturday, September 6, 7, 8, and 9, 1939; and Pennsylvania State College, Wednesday through Saturday, September 4, 5, 6, and 7, 1940. Newly elected officers are: President, John Frederick Dashiell; Secretary, Willard C. Olson; Directors, Leonard Carmichael and Gardner Murphy; Division of Anthropology and Psychology of the N.R.C., Edmund S. Conklin, Donald G. Paterson, and Christian A. Ruckmick; S.S.R.C., A. T. Poffenberger; A.A.A.S., Walter R. Miles and Christian A. Ruckmick. Harry F. Harlow was elected a member of the Committee on Precautions in Animal Experimentation.

The Association voted to continue affiliation with the Inter-Society Color Council for the year 1936-1937, and reappointed the present voting delegates and additional delegates. The Committee on Psychology of the National Advisory Council on Radio in Education reported that the Council is being discontinued and that the Committee should, therefore, be discontinued. It recommended, however, that the American Psychological Association should hold itself in readiness to coöperate with a

similar organization of broader scope that is now being set up. It was so voted. The report of the Committee on Psychology and the Public Service was accepted and the Committee continued. Two reports from the Committee to Study Problems Connected with the Teaching of Psychology were accepted with thanks and extra reprints were ordered to be sent to Junior Colleges and to High Schools. The Committee was continued with Goodwin B. Watson as Chairman and Calvin P. Stone and Arthur I. Gates as members with the suggestion that a similar survey be undertaken in Teachers Colleges. The report of the Committee on Motion Pictures and Sound Recording Devices in the Instruction of Psychology was accepted with thanks and the Committee continued with minor changes in membership, and with Milton Metfessel as chairman. The report of the Committee to Study the Qualifications of Industrial Psychologists was accepted with thanks and the Committee discharged.

A brief report of the first annual meeting of the Board of Editors and the Council of Directors was presented to the Association. A resolution was adopted unanimously commending the accuracy and efficiency with which Leonard Carmichael as Treasurer, Walter S. Hunter as Editor of *Psychological Abstracts*, and Herbert S. Langfeld as Business Editor of the Association's Publications have served the interests of the Association as revealed by the audit conducted by Ernst and Ernst, certified public accountants. The *Journal of Abnormal and Social Psychology* was authorized to issue a double number in the fall of 1937, to adopt an improved format, and to increase the size of its annual volume. The report of the Committee to Study the Problem of Costs of Continuing *Psychological Abstracts* for the Next Ten Years was adopted and the Committee discharged. This report indicates that the *Abstracts* should become self-supporting at the end of another four or five years. Careful consideration of the proposals submitted by the Committee to Study the Costs of Conducting the Affairs of the Association, led the Association to vote to continue the office of Secretary and to combine the Business Office of the Association's Publications with the office of Treasurer. This was accomplished by accepting the resignation of the Business Editor which had been submitted under date of December 12, 1936, to take effect prior to December 31, 1937, and electing Willard L. Valentine, Treasurer and Business Manager for the term 1937-1942. The Association also voted to authorize the Council of Directors to take the necessary legal steps to dissolve the Psychological Review Company.

The Association voted to censure an Associate Member who is alleged to be offering psychological diagnosis and treatment by mail.

The Association voted to affiliate the Society for the Psychological Study of Social Issues. The Association also placed on the agenda for final action at the 1938 Annual Meeting petitions for affiliation from the Midwestern Psychological Association, the Western Psychological Association, and the American Association of Applied Psychologists. Because the Clinical Section of the Association voted to disband, the Association voted to amend the By-Laws by deleting Article XII and renumbering Article XIII to become Article XII. The Association voted to appoint Henry E. Garrett and Walter R. Miles to serve on the Joint Committee of the Association, the Psychologists' League and the Association of Consulting Psychologists. A Committee to Prepare Examination Questions in Psychology was appointed with Ben D. Wood as Chairman, and F. C. Dockeray, Alvin C. Eurich, Richard W. Husband, Theodore F. Karwoski, Floyd L. Ruch, and Lewis M. Terman as members. The

Association appointed John E. Anderson as its representative in the newly formed American Documentation Institute.

A total of 130 scientific papers was presented in the twenty-two formal sessions. Papers on animal psychology and physiological psychology continue to predominate. A group of eight papers dealing with an experimental attack on problems of conflict and frustration suggests that this topic is passing from the domain of text-book theorizing to quantitative laboratory study. Vigorous research activity continues on a broad front as indicated by the number and variety of fields represented in the program. Pressure for place on the program was still great, though many papers on applied psychology were shifted to the meeting of Applied Psychologists.

The meeting of Applied Psychologists was held at the University of Minnesota on Monday and Tuesday, August 30 and 31, preceding the meetings of the American Psychological Association. It was attended by 466 people, and resulted in the organization of the American Association of Applied Psychologists. This group offered a program consisting of 9 formal sessions at which 58 papers on various phases of applied psychology were read. In addition, 5 round tables were held on Monday and 5 on Tuesday. At the organization meeting held on Tuesday evening, Dr. Douglas H. Fryer was elected President, Dr. Horace B. English was elected Secretary, and Dr. Edward B. Greene was elected Treasurer.

On Wednesday evening, 10 research and instructional films were shown. Three round tables on clinical psychology, one on psychological service in the federal government, and one on the selection of students for Teachers' Colleges were held. In addition, a Round Table on the Psychology of Learning and a Panel Discussion on Experimental Approaches to Psychoanalytical Concepts were held.

On Friday evening, the Presidential Address, "The Determination of Behavior by Stimuli—Past and to Come," was given by Edward C. Tolman. In considering the behavior of an organism at a choice-point in a maze, President Tolman spoke of the 'behavior ratio' as the relative tendency of the organism to make one of the two possible responses. He then listed and discussed the kinds of variables of which this ratio is at any given time a function—*e.g.* the variables of heredity, motivation, past behavior, or present 'hypothesis.' President Tolman used motion picture films to illustrate 'VTE' or vicarious trial and error in rat behavior, and defended anthropomorphism as a practical source of problems and hypotheses in animal psychology.

Following the Presidential Address, the University of Minnesota entertained members and guests in the foyer of Northrop Auditorium where a reception was held and light refreshments were served.

Excellent facilities were provided for the meeting. Professor Richard M. Elliott, his colleagues, assistants, and administrative officers and members of the staff of the University fully deserved the enthusiastic "official thanks" adopted at the Annual Business Meeting.

University of Minnesota DONALD G. PATERSON

THE INDIANA CONDITIONING LABORATORY

The recently completed conditioning laboratory of Indiana University meets a long-felt need in psychological and physiological research at this institution. As the name of the laboratory indicates, it has been designed principally for work on the conditioned response and related problems in the field of learning. The floor

area of approximately 960 sq. ft. is divided into 6 rooms, while two additional rooms totalling 370 sq. ft. are available as living quarters for an assistant or animal caretaker. A floor plan showing the general arrangement (exclusive of the living quarters) is given in Fig. 1.

The laboratory has its own hot water and gas systems, two sinks, a specially mounted tub for washing large animals, and cooking and sterilizing facilities. The

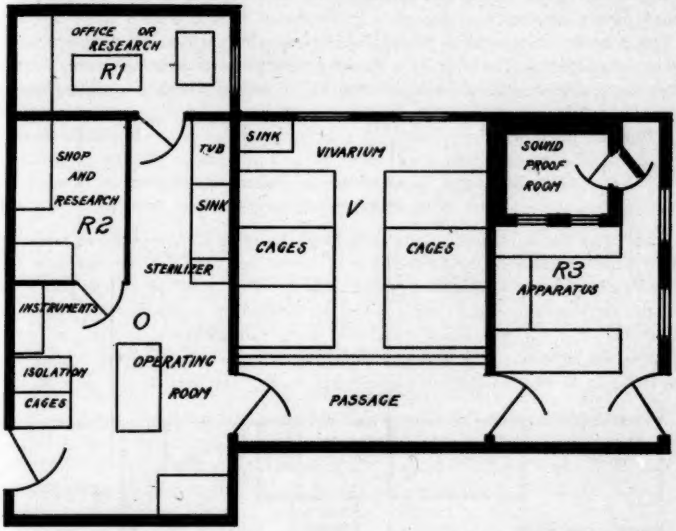

FIG. 1. FLOOR PLAN OF THE INDIANA CONDITIONING LABORATORY

working area has concrete flooring throughout. In the vivarium (V, Fig. 1) are six permanent cages, each of which is large enough to accommodate comfortably three large dogs. These cages are constructed of heavy woven wire and are firmly fastened to the walls and floor. They are adequate for housing cats, monkeys or young chimpanzees, as well as the dogs which now occupy them. Each cage is 6 ft. 3 in. in height to permit ample standing room, and is built on specially treated concrete which slopes steeply to large centrally-located drains. Hot, cold or 'mixed' water can be run by hose to any of the cages. Solid wooden shelves, on which the animals may sleep, are part of the cage equipment.

Research space is available in rooms R1, R2, and R3 (Fig. 1), although R1 is at present being used as an office, and a small shop has been set up in R2. A complete operating room with special illumination is also provided (O). In this room are an operating table, instrument cases, sterilizing facilities and two movable isolation cages.

Most of the apparatus is at present located in R3, within which has been constructed a small soundproofed chamber, 5 x 7 ft., where the subjects can be isolated

from distracting stimuli. R3 can be darkened so as to permit one-way vision through the two triple-glass windows of the soundproofed room. The latter can be illuminated by either alternating or direct current, and is electrically insulated for work with action potentials. Present equipment in R3 includes devices for producing various kinds of conditioned and unconditioned stimuli, timing equipment to control the length and sequence of the stimuli, a cathode ray oscillograph with high-speed camera, and special pneumatic apparatus for recording automatically the responses made by the subjects.

A program of research is already under way which will make extensive use of these new facilities. The immediate, though temporary, staff of the laboratory includes two psychologists, one physiologist and M.D., several graduate students, and an animal caretaker.

Indiana University W. N. KELLOGG

THE RESEARCH LABORATORY IN PSYCHOPHYSIOLOGY AT NORTHWESTERN UNIVERSITY

Last year the third floor of the Minerology Building at Northwestern University was made available to the Department of Psychology. The interior has now been completely re-built to serve as a research laboratory in psychophysiology. As shown in the accompanying figure, the quarters are entered by a private outside stairway. The animal room will house rats, cats, dogs, and monkeys, as well as certain obstruction apparatus. The histological and operating room is equipped for stereotaxic stimulation as well as ablation experiments. In the next section has been placed a

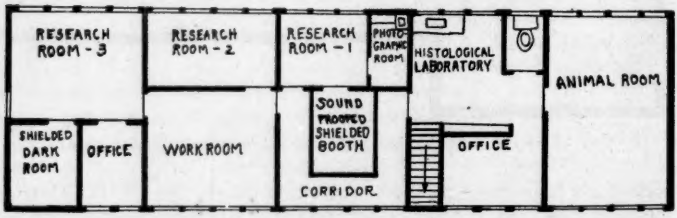

FIG. 1. RESEARCH LABORATORY IN PSYCHOPHYSIOLOGY, NORTHWESTERN UNIVERSITY

double-shielded, soundproofed booth for work with human subjects. The unit includes a special room for action-potential equipment and a photographic dark room. Measures such as skin resistance, blood-pressure, and metabolism may be taken by running leads from the booth to apparatus set in the corridor. By such means the subject is more thoroughly isolated from the recording equipment and its operation. If desired, an assistant seated above the booth may secretly observe and photograph the subject's behavior. Three other research rooms are provided, one for the experimental alteration of muscular tension, another for animal studies, and another for electroencephalographic recording. An assistant's office is located at this end of the laboratory and a large work room is equipped with desks for graduate students. All rooms are well lighted and the shielded booths have force-draft ventilation.

Northwestern University G. L. FREEMAN

BOOK REVIEWS

Introduction to Educational Psychology. By WILLIAM CLARK TROW. Boston, Houghton Mifflin Co., 1937. Pp. 417.

This elementary textbook has been written for students without a previous knowledge of psychology. It is designed primarily as a practical aid in the field of education. The subject-matter is, however, largely that of an elementary psychology, the reader being treated to brief discussions of such matters as emotion, intelligence, sensation, perception and meaning, motor response, and the acquisition of knowledge. There is shown throughout a casual familiarity with the psychological names and enterprises of the last few decades. This type of running comment, while somewhat intriguing to the college student, does not lead to a satisfactory integration of materials. One misses the vigorous forward push that might be given to educational psychology, especially at the introductory level, by a single devotion to modern, well-documented principles of motivation and learning.

In a few instances, the author's obvious intention to keep on a conversational basis with his readers, leads to the assumption that difficult concepts may somehow be made simple and palatable if treated briefly and lightly. For example, Spearman's elaborate theories covering the general factor, specific factors, the two factor theory, the eduction of relations, and the eduction of correlates are all treated in exactly one page. Such treatment, far from being a simplification, is nothing more than honorable dismissal. At the most, it could be expected only to turn inquiring students toward Spearman's original work, and if they do so turn, what happens to them? Obviously they will make the painful discovery that what was dealt with so blithely in a sentence or two is something beyond their present interest, training, or comprehension. Again, Herbart's classic five steps of instruction are simply listed by name. It is unlikely that any elementary student would have any sense of the meaning of such terms. One feels that either they should be left out completely or developed in some detail in relation to their impact upon modern methods of teaching and learning. Similarly with respect to Trow's treatment of statistical methods: he does not confine himself to the building up of concepts, but gives graphs and detailed descriptions which almost, but not quite, permit the student to learn how to perform some of the simpler operations. It would be unfortunate if some of the oversimplified statements were accepted literally by the beginning student. For example, one reads that "such physical characteristics as stature, and color of the skin, hair, and eyes, and in many cases intelligence and temperamental factors, are inherited" (p. 354). Even in the context it is hard to understand the meaning of that sentence. And one reads that "the intelligence quotient of children from good homes usually drops somewhat after the first testing, largely because they have lost the initial advantage of a better than average opportunity to learn the words and tasks called for on the tests. Similarly, the IQ of children from underprivileged homes frequently shows a rise" (p. 260). No mention is made of recent studies which show that children remaining in underprivileged homes nevertheless tend to show a decrease in their IQ. In fact little mention is made in the section on childhood and youth of the substantial contributions issuing from eight or ten research centers over the country.

To a certain extent the criticism above penetrates Trow's whole volume. Within the chapters, the treatment is indeed simple, and frequently almost gay. Trow has

moreover confused simplicity with brevity; that he has erred in his zeal for sugar-coating thus encouraging smattering and mere verbalization on the part of the student.

One may always ask, but what is an author to do? There are only a few pages, and the field is broad and deep. One obvious suggestion is that much of the older materials on such matters as phrenology, illusion, and superficial neurology might well be reduced or eliminated; in short, that the author's funnel be turned around so as to include a great number of observations, principles and applications emerging from present-day research activities, with a corresponding shrinkage of materials from the past.

Within his lights, Trow has done a good job. If one must begin in the past, moving along step by step with the citing of names, errors, and theories as they have bobbed up in psychological activity, then the reviewer would be the first to grant that Trow knows his way about. But the price often paid for this is paid once more in Trow's volume; namely, there is then little space for an informed, sympathetic, and original account of what is going on in the world today.

University of Iowa GEORGE D. STODDARD

General and Social Psychology: A Textbook for Students of Economics and of Social Sciences. By ROBERT H. THOULESS. 2nd ed. London, University Tutorial Press, 1937. Pp. xii, 522.

The first edition of this work was originally written as a text to prepare for the examinations leading to the degree of bachelor of science at the University of London. While entitled 'Social Psychology,' it dealt mainly with subjects in general psychology and to a lesser degree with phases of social or economic psychology. The emphasis was largely, however, on practical problems.

The present work is considerably expanded and radically changed. Chapters XVII and XXI are entirely new. They were added to meet the criticisms of reviewers to the effect that the nature of group reactions in society and the class system required modern treatment and that statistical measurements are helpfully discussed in connection with educational phases of social psychology. The author also calls attention to a major change in the book in view of the fact that he now uses the term instinct in a much more restricted sense than before. He believes that the underlying concept should imply less fixed inherited patterns of behavior in the case of human beings.

The twenty-five chapters carry the student from the introductory definition of psychology as "the positive science of human experience and behavior," a discussion of methods, on to the innate and acquired patterns of behavior, the emotions, sentiments, and attitudes leading to the integration of personality, the foundations and transformation of human behavior, hunger, fear, sex, acquisitiveness and pugnacity, laughter, play and work, perception, thought and language, conflict and volition, social behavior and grouping, the psychology of morality and of economic value, statistics, mental testing and intelligence, and close with a discussion of the psychology of esthetics, and of scientific and religious development.

Thoughout the book the author adheres to the dual approach to human conduct. Credit is given especially in animal and child psychology to the behavioristic techniques which have developed a systematized study of responses. When the discussion of responses begins, we find too that "psychophysical dispositions, which cannot be directly observed, are postulated in order to account for the complex patterns of

habituated and instinctive behavior." Intelligent behavior is equivalent to variable responses elicited by the changing requirements of the situation and therefore not specifically contrasted with instinctive behavior which is also somewhat modifiable. The laws of exercise and of effect are discussed at length and numerous experimental data are brought to bear on the exposition throughout the book. In connection with the S-R interpretation, the reviewer has failed to find the presentation of Woodworth's more recent modifications of this form.

The discussion of emotions proceeds largely along classical lines. Much of the more recent research has been omitted and emotions are called 'driving forces,' inborn dispositions to respond, which serve "intelligent variable behavior just as the automatic instinctive responses serve the needs of stereotyped behavior." Also according to the best British traditions sentiments are classed as dispositions to feel emotion. They develop out of the more primitive impulses but the latter are still imbedded in the more complicated mental pattern. From the treatment of this and other subjects it is clear that the text has a distinctly social flavor, so that when sensation and perception are presented it is a relief to find a relatively short account. Here we are pleased to note a fairly full discussion of the more recent phenomena like the constancy, form, ambiguous figures, and the "modes of appearance" of colors.

A notable feature of the book consists in bringing in pathological phenomena to illustrate deviations from normal conduct. The passage from thought through conflict to volitional phenomena is a case in point. Thus when awareness of membership in a group leads to the discussion of loyalty, group leadership, and manners, a transition occurs toward the present abnormal social unrest in the stratification of society and in the class conflicts which result in socialism or fascism. Students of present international movements will do well to read this inserted chapter (XVII). The other entirely new chapter (XXI) on the use of statistical methods is limited to those most currently used in connection with social phenomena. The topics treated include sampling and its standard error, the difference between two means, correlation, and reliability of test results. While the descriptions and illustrations are excellent, condensation required the omission of important formulae and their evaluations. For the same reason, while factor analysis is discussed in detail (but not indexed!), the reader is referred in the text to Thurstone's book for more extensive treatment.

The book closes with a competent and exceedingly interesting presentation of esthetics as a form of social appreciation and production, on the one hand, and a discussion of scientific curiosity and discovery together in the last chapter with a rather brief account—for a book primarily social in character—of religious attitudes and problems. Significant to a modern student of social phenomena are the statements: "The truth seems to be that religious institutions for various reasons have ceased to be important social integrating forces. Similarly, religious differences are ceasing to be important segregating forces."

The reviewer regrets that books like this one are not more readily available to American students. While there are no figures and, with possibly only one exception, no tabulated or diagrammed materials, the diction is clear, interesting, and replete with citations from the professional literature. Over 300 footnote references are gathered together at the end with the foreign sources in translated form. The index is the combined subject and author type. A logical progression is provided, but not with too obvious protrusion. The work is one which the science of

psychology must reckon as among the few best social psychologies that have been written with authority by well-equipped psychologists. It should be also on the shelves of professional men in many allied fields and, with the omission of perhaps a score of pages, it can be profitably read even by the layman.

University of Iowa CHRISTIAN A. RUCKMICK

Visual Perception. By M. D. VERNON. Cambridge, University Press, 1937. Pp. xi, 247.

In the relatively short space of two hundred-odd pages, Miss Vernon gives us excellent and concise digests of a large number of experimental titles in visual perception. In addition, the author has organized the facts into something more than a bare catalogue of researches. She has followed her avowed purpose of presenting the facts "as far as possible without preconceived theoretical bias," but at the same time she has made use of certain heuristic devices for organizing and classifying these facts. Using these devices, she is able to bring together around a common theme a large number of scattered researches. For example, we find such heterogeneous topics as brightness and color constancy, time-errors in successive comparison, the orientation of the spatial framework of perception, and standards of 'absolute judgment,' all brought together under the common heading of the 'function of the ground.' The reader may not always agree with the allotment of facts to such topics or principles, but they furnish a convenient framework upon which to exhibit the facts and often furnish a context of new significance for the results.

The book is divided into four main sections: (1) the perceptual process; (2) the relation of perceptual content to "affective and quasi-affective states" (set, task, attitude, attention, and the like); (3) the objective structure of the perceptual field; and (4) the development of perception in children and the problem of perceptual types. Of these four headings, the third is the longest and the most adequately presented, possibly because it represents a condensation of the more factual portions of *Gestalt* investigations and of other phenomenal approaches which constitute such a large bulk of our modern literature on perceiving.

The first topic, that of perceptual process or function, has received scant and inadequate treatment in the experimental literature. This state of affairs is reflected in Miss Vernon's presentation. Following a preliminary characterization of perceptive process which exhibits a marked associational flavor, she turns to the experimental results on the 'stages of perception.' These stages are the ones which usually come out of tachistoscopic experiments, such stages as 'vague awareness,' 'generic object,' 'specific object,' and 'understanding of meaning.' Even if we could consider these artificially abstracted 'stages' as representative of the temporal course of perceiving (which the reviewer doubts) we should still have an extremely narrow treatment of the functional character of this activity.

Such topics as task, trend, attitude, and set are considered only as 'states of the organism' which determine the characteristics of perceptual content, rather than as aspects of perceptual activity itself. Yet such terms are by their very nature functional and lose much of their significance when taken out of a functional context. They cannot be understood simply in terms of their effect upon perceived objects. Attention, it is true, is treated functionally and the author has emphasized the advantages of such an approach over the structural approach. Attention is ap-

parently conceived, however, as a *separate* process divorced from perceptual activity itself; it is a 'focusing of awareness' upon certain sensory processes.

The shortcomings in such a functional treatment are not, however, to be laid at the door of the author of this particular book. This narrow and loose approach is inherent in the bulk of our literature on perceiving. In many researches the existence of perceptual activity or process is simply ignored.

Turning to the phenomenal problems of the third and largest portion of the book we find the treatment more complete, better organized, and, in general, highly adequate. There is, for example, an especially fine chapter on space perception. Here the author lays stress upon the coöperation and integration of the various factors in space perception rather than upon artificial fractionation and isolation. Thus we get a totalized rather than a piecemeal picture of the operation of such conditions as retinal disparity, convergence, accommodation, and the various 'secondary' factors. Other chapters dealing with configurational organization and related problems are similarly adequate and coherent.

We are struck again by the lack of functional material when we turn to the concluding chapters on the development of perceiving and the problem of perceptual types. The chapters are careful and critical summaries of the available research, but the chapter on development might more properly be called "The Development of Perceptual *Phenomena* in Children" rather than "The Development of Perception in Children," since practically no mention is made of perception as an activity.

Visual Perception is a book which should be of much value to the student who has already acquired a moderate first-hand acquaintance with typical experimental studies in perceiving. The book should serve to widen the student's knowledge of facts and to aid him in organizing what he already knows, while his previous knowledge of techniques will serve to fill out the brief accounts of procedure given by Miss Vernon. The appendix, containing descriptions of the major kinds of tachistoscope, will serve as a substitute for a first-hand knowledge of this important device for the study of perceiving.

Cornell University T. A. RYAN

Naven. By GREGORY BATESON. Cambridge, University Press, 1936, Pp. xvii, 286, 28 Pl.

Naven is the name of a peculiar ceremony of the Iatmul people who live on the Sepik River in New Guinea. In this ceremony a *wau* (mother's brother, real or classificatory, sometimes other relatives participate) does honor to his *laua* (usually sister's child, real or classificatory) when the latter for the first time does some act that is regarded as meritorious—it may be the catching of a fish or the committing of an homicide (the Iatmul are a head-hunting people). The *wau* dresses himself in women's clothes and with some histrionic ability burlesques the behavior of a woman. He thus makes a comic spectacle of himself as he searches for the *laua*. When the *laua* is found the *wau* makes it a present of some food and eventually the *wau* is given a present in return. There are other subsidiary acts in the ceremony but this account will suffice.

The author with an eye to the explanation of this ceremony heuristically applies three different approaches to the study of the culture as a whole. The first of these he calls the "structural" point of view. It is really a descriptive account of the

behavior of the people, generalized, of course, but clouded by an epistemological setting and vocabulary which seems unnecessary. The result of this technique is the discovery that the "cultural structure provides a setting with which the ceremonies are consistent." The second, he calls the "sociological" point of view. Its object is the discovery of the factors which contribute to the integration of the individuals in the social groups. These are found to be certain classificatory relationships (the affinial), and they play a part in the Naven ceremony.

The third approach is called the "ethological." It derives from the notion of intellectual and emotional types of culture as set forth by Benedict in *Patterns of Culture,* and Margaret Mead in *Sex and Temperament.* The notion is that every group has an intellectual and emotional pattern, an eidos and an ethos, the two together constituting a *Gestalt*-like configuration. It is a descendent of Dilthey's and Spengler's *Zeitgeist,* and it is at once the cause of social behavior, and is revealed by the social behavior.

The author defines the eidos as "the standardization of the cognitive aspects of the personality of the individuals." The standardization is effected by the stimulation of the cognitive acts; native endowment, *i.e.* intelligence, is not involved. In other words individuals remember and think (these are the cognitions mentioned by the author) according as they are stimulated by the culture, and since the cognitions of many individuals are stimulated by one culture the cognitions get standardized. He finds the eidos of the Iatmul people to be characterized among other things by a tendency to construct an enormously complex cultural-structure, to classify relationships in dual fashion, to much argumentation about totemistic ancestors, and to develop a memory for classificatory names. Apparently, however, the eidos makes little contribution to the Naven ceremonies.

The ethos is differentiated sexually. The men tend to be excessively proud, boastful, dramatic, over-compensated, and violent. The women are jolly coöperators, serene, and kind. The two ethoses are thus in contrast, and the men at least are scornful of the emotional traits of the other sex. In the Naven the doing of honor is more in accordance with the ethos of the women than of the men. Consequently the man dresses in women's clothes and, to cover his shame and at the same time to gratify his love for the theatrical, he burlesques the part.

The psychological interest in this theory is the attempt to ascribe a personality to a social group. A group-personality must, we presume, consist only of the personalities of the individuals that constitute the group. Since, as psychology is finding, individual personality-traits change with the age, history, and physical condition of the organism, and also with the situation in which the organism happens to find itself, the attempt to establish a group-personality would seem to present insuperable difficulties. The author's attempt is naïve; he apparently knows nothing of the psychological history of temperament, personality, and type; and is unaware that psychology no longer finds such dichotomies as "intellectual and emotional" descriptive of the facts. It would have been better to have recognized with Malinowski that every culture by tradition evaluates certain forms of behavior, and that in practice every individual of the groups tends, with wide individual variation, to conform to the tradition. If this view does not at once supply the author with the motivation which he seeks, he might discover that although there is group-behavior, as, *e.g.* in a ceremony, the motivation is always individual and is probably always multiple and not single.

The book as a whole is marked by earnestness and sincerity. The anthropologist will no doubt find much that is informative, suggestive and useful. The thought and the exposition is clouded, however, by the author's inability to accept a method as a tool without bothering too much about its epistemology and psychology. He might, for instance, have inferred a tendency among the Iatmil men to pride, bombast, etc., and methodologically have applied the inference to an explanation of the Naven without attempting the construction of an ethological theory.

Cornell University H. P. WELD

The Psychology of Social Norms. By MUZAFER SHERIF. New York, Harper & Brothers, 1936. Pp. xii, 249.

Sherif approaches social psychology through a consideration of social norms, the frames of reference in any given society by which social phenomena are gauged and governed. He makes clear that norms are peculiar to a given social group, and that the same norms are not to be given to the same events in different cultures. Since norms, once firmly established, are effective with the individual impressed by them, it is to be expected that the social behavior of persons in different groups will notably vary.

The author insists that these norms are at first external to the organism and may appear as components of "stimulus-situations," to be subsequently "interiorized" by the individual. This assertion, made in spite of the protest that norms are "not stable entities," suggests an existential nature for customs, traditions, and all social standards. It is difficult to comprehend this dualism of norm-as-stimulus and norm-as-interiorized.

In an effort to demonstrate how a norm is formed, is adopted by, and persists in, an individual, Sherif—following an old procedure—used the autokinetic effect exhibited by a point of light in darkness. Forty subjects were instructed to report the apparent amplitude of the autokinetic movement during a 2-sec. period. A part of the *Ss* observed first individually and then in small groups wherein each heard the reports of the other members; the remaining *Ss* first made group-observations and then observed singly.

Ss first observing in isolation "established a range and norm within that range" which was peculiar to each. In the subsequent grouping the reported ranges and norms tended to converge, although the convergence was not so close as when *Ss* first observed in the group situation, when "a range and norm within that range was established which were peculiar to the group." If, in the initial group, there is a rise or fall in the norms established in successive sessions, that is called a group-effect; *i.e.* the norms of the individual members "rise and fall toward a common norm in each session." In each group, leaders appeared whose estimations were followed; but once the group-norm was settled, the leader was ignored when his estimations deviated from that norm.

The author's interpretation of this exercise is not entirely satisfactory. He claims that his data "show in a simple way the basic psychological process involved in the establishment of social norms." Actually, there is little information concerning 'psychological process,' although the *Ss* were asked to record their 'introspection.' The chief result from these written notes was negative, the *Ss* contending that their estimations were not influenced by the oral reports of others.

Sherif asserts that the exercise (he calls it an experiment) is "an extension into

the social field of a general psychological phenomenon that we found in perception and in . . . other psychological fields," *i.e.* the organization of experiences about a frame of reference, which he describes in the language of the configurationist. But his adjective 'social' does not really set apart his 'objects' from others. He himself does not define the term 'social' and he fails really to differentiate the alleged 'social occasion.' The mere presence of two persons in physical proximity seems, for him, to guarantee sociality.

The fact that a norm is peculiar to the group which establishes it seems to Sherif to imply that "supra-individual qualities arise in the group situations." The notion of supra-individual products is, in the reviewer's opinion, a bit of magic which any system of social psychology might well do without. Again, Sherif believes that "cultural products have a reality of their own, independent of this or that individual member of society." This, to our way of thinking, is utterly inconceivable and is not to be made plausible by the assertion that "this immediate reality (of independent cultural products) may be verified easily by the resistance one meets when he deviates . . . from the well-established norms of his time." Where is the "resistance" encountered if not in individuals? How can one know that one is breaking with tradition except through the books, gestures, speech, and the like, of other individuals?

After the social interpretation of autokinetic movements, the book goes on to a more or less philosophical discussion of social values and basic needs. Values are found to be norms with affective properties; attitudes are the "psychological process(es) in the individual" resulting from the assimilation of values which must first appear on the side of stimulus. In other words, "value is the content of attitude"—if that is enlightening. It is noted that social values are so effective that they may markedly alter such activities as the procuring of food or the satisfying of sexual desires. With the aid of Piaget, Baldwin, and others, Sherif composes a final chapter concerning the ego, "that which is social in man." Its "chief constituents are values." "The ego formation is really the anchoring of ourselves in a frame with more or less definite relations; . . . this anchoring is *status.*"

The above quotations show how far Sherif drifts from considerations of an "experimental foundation" for social psychology which was promised his readers in Gardner Murphy's introduction to the book. The experimental foundation itself turns out to be chiefly the popular alone-grouped exercise of the class-room and test-cubicle variety which many would hesitate to call 'experimental.'

Sherif's emphasis on the social norm as an important determiner of psychological operations and their issues is in order—if not new. His admonition to writers to eradicate "community-centrism" or bias from their own particular social backgrounds is not amiss, and his illustrations from anthropology are interesting and pointed in demonstrating the varieties and the effects of social 'norms' or estimations.

Colorado College MARTHA LOU LEMMON

Readings in Educational Psychology. Edited by CHARLES E. SKINNER in collaboration with twenty-six others. New York, Farrar & Rinehart, 1937. Pp. ix, 630.

This volume of readings, like all such volumes, is intended as supplementary reading. The materials compiled are said to satisfy the new viewpoints in the subject that have "grown out of recent research and experimental studies," as shown in *Educational Trends,* volume III, 1934. As reported, the significant contemporary

trends in the subject are the following: (1) A marked tendency away from the emphasis on heredity in general and instincts in particular. (2) Less emphasis on the so-called laws of learning as an important topic in educational psychology. (3) A tendency away from the purely psychological and more emphasis on the educational aspects of the subject. (4) Increased emphasis on the topic of growth. (5) More attention to motivation and adjustment. (6) Effort to apply the psychological prinicples to the methods and materials of the classroom.

The volume succeeds in steering clear of the instinct controversy, although a reasonably good argument might be made for acquainting students with the problem in supplementary readings, even if for no other purpose than to dissipate some of the popular misconceptions. But a much stronger argument can be made for the inclusion of materials dealing with the reconciliation of "instinct versus learning." Yet this too has been omitted.

The heredity-environment problem, as such, is not given an independent chapter, but relevant quotations are, of course, included here and there; as, for example, in Mursell's chapter on "Growth," Sandiford's on "Acquisition of Skills," Schoen's on "Emotional Development and Control." One may question the wisdom of excluding from supplementary—and presumably more advanced—study a more or less representative presentation of this topic which has been and many believe still is of great importance. However, the editor states that the materials included were to be in harmony with the facts revealed by *Educational Trends;* and in this success has been achieved, so far as heredity and environment are concerned.

Fortunately, the amount of space devoted to learning does not harmonize with the second tendency given above. While the reader is not belabored with long arguments on the "laws" of learning, pro and con, the volume does contain four chapters—totalling about 100 pages—on the subject of learning. In these it is inevitable and proper that the "laws" of learning should receive some space, as well as the ways, conditions, and factors of learning. Here again, one might suppose that the so-called laws should receive more than casual treatment, since the learning process certainly is significant in the classroom. If, on the other hand, as indicated by (6) above, methods and materials of the classroom were to be stressed, the chapters on learning might have been more pronounced in those matters. These criticisms do not, however, detract from the educational significance of what has been included in these chapters.

The third tendency, noted above, has been rather well met; for very few of the editors were content, where relevant, to omit quotations dealing with the educational implications of their topics.

Although this volume probably contains more items than other readings on the growth of behavior and of psychological functions, there does not appear to be the emphasis upon growth that one might expect from (4). In fact, there is a surprising paucity of items dealing with the development of behavior from the pre-natal period to maturity and through senescence, especially during infancy and childhood.

Adjustment—a psychological term now enjoying great favor—receives a reasonable amount of attention; for it is the principal concern of three chapters. But motivation, joined with adjustment as deserving more attention in educational psychology, is treated only casually and, it seems, inadequately.

The sixth tendency (6) receives more than the usual amount of attention, and properly so, in five chapters entitled: "Educational Measurements," "Diagnosis and

Remedial Teaching," "Individual Differences," "Intelligence and Its Testing," and "Psychology Applied to the Modern School."

It is highly improbable, of course, that any two persons arranging a chapter of readings on a given topic would select entirely the same excerpts for quotation. But whatever excerpts are selected should be of recognized merit. This, for the greatest part, is true of the present volume. Nor would any two editors, arranging an outline of readings, agree entirely with respect to the topics to be included and their order of presentation. The topics of this volume, however, "cover the field."

Some persons question the value of any book of readings, maintaining it is best to go directly to sources, and that one should select his own sources with a view to meeting the particular needs of his students. Others, however, find such volumes valuable. But assuming that a volume of readings, is desirable, one major objection, as a matter of personal opinion, is to be made against the book under consideration. Rather than such a very large number of brief excerpts, it would be better to use fewer but more complete quotations. This would make the treatment less eclectic, to be sure; but it would make possible a more nearly adequate and sequential presentation of a topic, in place of the disjointed character of numerous quotations which in themselves are often only beginnings. However, disregarding this criticism, it will be found that the present volume of readings is among the best in the field.

Cornell University FRANK S. FREEMAN

A Black Civilization: A Social Study of an Australian Tribe. By W. LLOYD WARNER. Introduction by Robert H. Lowie. Harper & Brothers, New York, 1937. Pp. xviii, 594.

Two admirations develop early and persist throughout the reading of this book. First, one gains a genuine admiration and respect for the creative intelligence of the primitive people who developed and maintain such an intricate structure of social relations that the modern "civilized" person would be lost in them. In the second place one is impressed with the competence of the investigator who comprehended and so ably presented a picture of this elaborate structure.

This work is no mere cataloging of technological objects and practices, anthropometric measurements, kinship nomenclature, magical practices, myths, warfare habits, etc., with no other correlation than the statement that they "belong to the culture of the people." It is an integrated picture of a culture in which each aspect is shown to be functionally related to the whole. Magic, kinship structure, ritual, economic behavior, are all shown in their neatly articulated interdependence. In his research Warner has not only shown unusual skill in applying the functional theories of Radcliffe-Brown and Malinowski, but has contributed important refinements in method and interpretation (*e.g.* he has expanded the use of free association methods in establishing functional relations between larger culture complexes such as the myth and ritual in the totemic complex and the environmental facts such as the seasonal rhythm).

It is highly significant that by using the functional approach the author has been able to make what is perhaps the clearest analysis of an Australian culture available to date. So far as the sociologist is concerned, the report will rank as one of the most satisfactory anthropological documents available; indeed it sets a mark toward which sociological description of "modern" culture may well strive.

The Murngin people are one of eight tribal groups inhabiting the northeastern

part of Arnhem land, which is in northern Australia bordered by the Gulf of Carpentaria and the Indian Ocean. The climate is tropical. The technological culture is roughly somewhere between the Old and New Stone Ages in our own chronology; and is a more or less minimal adaptation to the annual cycle of the rainy season of scarcity and the dry season of plenty.

The fundamental basis of society is an elaborate kinship system which recognizes and specifies distinctive reciprocal behavior for seventy-one classificatory relationships. The lineal relations that link five generations are very strong; the lateral lines—three on the father's side and three on the mother's—are less powerful; but the Murngin have contrived an intricate reciprocal complex that cuts diagonally across the lineal and lateral relationship lines and binds the whole fabric firmly.

The kinship groups are organized into patrilineal exogamic clans which have local territories. The sixty odd clans are grouped into two moieties which intermarry. The tribal grouping is of no social importance. There is no tribal organization or control. The clan is the war making group, though occasionally several clans may become temporary allies for war purposes.

Women and uninitiated males are ritually in the lowest social status. Women remain in their lowly estate and occupy a relatively simple social rôle. All males rise in the social organization through a series of *rites de passage* which start with the father's mystic dream in which he transports the child's soul from the totemic well (home of the totemic ancestors) to the mother's womb. (Contrary to the general opinion the Murngin know the part the male plays in the physiology of conception, but they regard it as of little or no significance compared with the mystic conception.) The ritual progress of the male through the age-grades stops with his mystic transportation back to the totemic ancestors at the time of his death. In the intermediate stages he occupies an increasingly elaborate social rôle. The author notes a marked difference between the sexes as to richness and complexity of personality. He attributes these differences to the variance in social rôle.

Warfare is an important functional part of the social process. "Without it Murngin society as it is now constituted could not exist." It limits the male population to a size compatible with the polygynous family form and attendant culture complex. It serves as an antithetical force to wider cultural solidarity and strengthens clan solidarity, thus serving to support the bonds that hold individuals in their reciprocal cultural positions. There are six different patterns of warfare, each with a different set of rules and proscriptions. The most disastrous and least used form is the *gaingar* which involves groups of clans in a general war. When one set of allies invites another to participate in a *gaingar* the people say it is a spear fight to end all spear fights and that it will put an end to all feuds and wars and bring final peace.

By an ingenious use of the method of free association on many persons, the investigator has established the intimate and real functional relations of the rhythm of nature, the myths, the rituals, the social organization and the *Weltanschauung* of the Murngin.

In a study of magic, Warner shows how the effectiveness of black magic depends on its redefining the social rôle of the person so that his group can no longer deal with him as anything but a dying person ready to be transported to the totemic spirit world. If white magic can restore him to his place in the social structure, his group again treats with him in his accustomed functional position and his

health returns. The author's efforts to show that magic and religion in the sense of Durkheim's conception of the "church" are the same will doubtless be challenged by social psychologists, but his sociological interpretation of magic is an important contribution to the sociology of suggestion.

A valuable appendix of supplementary material completes the work. Sociologists will find the book a significant contribution to functional theory of social dynamics.

Social psychologists will also find the book of major importance. Some of the contributions to social psychology are on the following points: (1) the personality results of simple and complex rôles in the same culture; (2) the free association method of studying social meanings and cultural integrations in personalities; (3) the sociology of suggestion; and (4) the social basis of logic. The work lacks, as most studies of primitive peoples almost necessarily lack, a full length study of individual personalities. The author promises us, however, one of those rarities of anthropological studies, an autobiographical account of a Murngin man as he experiences his passages through the age grade rituals. This document is now in preparation and will be eagerly awaited by social psychologists who read the present report.

Cornell University LEONARD S. COTTRELL, JR.

General Psychology: An interpretation of the science of mind based on Thomas Aquinas. By ROBERT EDWARD BRENNAN. New York, Macmillan Co., 1937. Pp. xxxvii, 509.

As the subtitle indicates, this book is frankly philosophical psychology or even psychological philosophy rather than psychology *per se*. Justification for such a treatment is put forth in a prefatory essay by Dr. Rudolf Allers of the University of Vienna. There the writer, although granting that psychology is a factual science, urges that it is a "peculiar kind of science." "If [the physicist] feels a desire to develop a philosophy of physics, he ceases automatically to be a physicist and become a philosopher. . . . But, to repeat, with psychology the case is different." To the reviewer the first statement is precisely as true of the psychologist as it is of the physicist. The presentation in the body of the book demonstrates this by being philosophy not psychology.

The text is divided into a prologue and three books. The prologue presents the system of Thomas Aquinas in so far as it involves the material now considered psychology. Book I deals with "Organic Life," Book II with "Sensory Life," and Book III with "Intellectual Life." Each book is subdivided into two sections, in the first of which the topic is examined for its factual material, and in the second, for its philosophical interpretations. For example, Section 1 of Book I is entitled "The Science of Organic Life," and Section 2 is called "The Philosophy of Organic Life." This organization of the material is continued throughout the book. Thus the treatise becomes an admirably complete and logically constructed philosophy incorporating the facts of psychology and biology in so far as they are useful to and in accord with the system.

That the facts presented in Section 1 of each book are largely determined by their application to the interpretation of Section 2 may be illustrated in a few instances. Sensation, we are told, is a "vital operation issuing in knowledge" and the several attributes give the specific aspects of that knowledge. Such a notion of sensation as an epistemological entity has been challenged, long since, for purposes of

scientific psychology. In the laboratory sensation is no more, certainly, than an abstract category under which observational data are listed. "Without doubt the most numerous of all sensations for the average person are those of color. And yet, a purely psychological description of them has never been made." (Sic!) What is more psychological than the description of an object in terms of color?

"Perception implies a *knowledge of sensations* on the part of the *sensus communis*." The latter is an "inner sense" postulated by Thomas Aquinas to which he ascribed the "power of sifting, sorting, and unifying the data of sensory experience into perceptual wholes." Here is a true "bundle hypothesis" for the Gestaltists to shoot at. It indicates no recognition of the notion of perceptions as integrated compounds, that has grown up from experiments with concrete perceptual objects, because such a concept could not be assimilated by the predicated system. Examples might be multiplied but the picture would not be changed. Biologists will not rally to the interpretation of evolution, nor physicists to that of the principle of uncertainty.

How one values Professor Brennan's presentation and interpretation of psychology will depend upon his willingness to accept the systematic postulates. As we have indicated, the response of the laboratory psychologist will be negative. But, then, the latter is but slightly considered in most of the recent textbooks of psychology, of which all but a scanty few present some explanatory system or sociological scheme bolstered by such factual material from psychology and the other sciences as suits their purposes. They do the same sort of thing that Professor Brennan has done, only not so well. In either case experimental psychology is the loser.

Hobart College FORREST LEE DIMMICK

Theory and Art of Mysticism. By RADHAKAMAL MUKERJEE. London, Longmans, Green & Co., 1937. Pp. xvi, 08.

When psychologists wish to characterize an utterly unscientific mode of explanation they frequently use the appellation (or epithet) 'mystical.' Whether they are aware of all the implications of the term is not always clear. At any rate, for those psychologists who happen to be seeking a current and, to a degree, adequate treatment of the mystical mode of interpretation this book should prove to be a *vade mecum*. Its author, a professor of economics and sociology in Lucknow University, has performed a real service for occidental readers, regardless of one's ultimate estimate of the applicability of mystical ways of thinking to problems of human nature.

One would seek vainly, however, for a critical evaluation of mysticism. Despite the author's avowed intent "to present an objective explanation of the mystical life and experience," one notes from the beginning that he accepts without question the validity of mystical interpretations. His "objective explanation" is of the nature of a defense of mysticism by way of deducing "certain universal principles" from the varieties of mystical expressions found in the numerous religions of the world as well as in the individual verbalizations of representative mystics. To augment his defense he marshals whatever data seem available from various sciences. Of interest to psychologists is the use he makes of the Jungian concept of the subconscious as well as of the Gestalt concepts of totality and structure. At the same time he is critical of those psychological studies which attempt to explain mystical experiences as abnormal phenomena. Such studies, the author contends, are an outcome of a

wrong approach to religion (*e.g.* Ribot, Freud, Leuba), for they envisage religion as an aberration, inevitably so when they look only to its bizarre features and ignore its more essential character as an agency of social value and cultural progress. Through the entire book runs the contention that religion and mysticism find pragmatic validation in terms of elevated social values and individual sanity. Of interest to physicists as well as to psychologists is his appeal to "distinguished authorities in modern physics and mathematics" who "now seem to regard consciousness as fundamental and matter as derivative from consciousness." Accordingly, he relates (p. 269) Jeans' view of the university "as consisting of pure thought" and Eddington's belief that "the stuff of the world is mind stuff" to the Yoga conception of the universe as "mere thought."

Let not the reader infer from this review that Professor Mukerjee puts science on a par with mysticism. Despite his readiness to invoke science in defense of mysticism, he does not attempt to serve both masters on an equal footing. Mysticism is the preferred master; science belongs to a lower order. Hence the implications of the second law of thermodynamics need provoke no qualms anent personal security. For we find in eastern mysticism "something more than anthropomorphic systematization;" something which "alone can rescue metaphysics from the crippling effects of the invasion of physical science, and save human souls from the laws governing the motion of electrons and protons or the ceaseless flux of the evolutionary process." Indeed, "science has not always given a true and unfaltering guidance for the direction of human activities along socially desirable channels. Thus, instead of science being called on to advise what expressions of the religious impulse will be permissible, it is upon rleigion that society will have progressively to depend for pulling the chariot of man's destiny along the path of progress."

A careful reading of this book has not convinced the reviewer, at least, that mysticism is more dependable than science "for pulling the chariot of man's destiny along the path of progress"—if by "progress" one means escape from obscurantist interpretations. But, then, the reviewer is doubtless seriously deficient in those habits of thought by which mysticism flourishes.

Lawrence College J. H. GRIFFITHS

Biologische Typen des Menschen und ihr Verhältnis zu Rasse und Wert. By EDUARD ORTNER. Leipzig, George Thieme, 1937. Pp. 104.

An attempt to clarify the ambiguities in Jung's "introversion" and "extraversion" led the author to develop his own schema for classifying human types on the basis of the relation of the organism to the environment, and to coin eight new words to describe his six a priori human types and the two main groups into which he divides them.

The first three types adapt their environments to themselves. Dr. Ortner calls them the intrabasal types, in contrast to the last three, or extrabasal types, which become adapted to their environments. For each extrabasal type there is an intrabasal type that contrasts with it in some ways and resembles it in other ways. The whole schema is very pretty—far too pretty to fit the complexities of the actual world of real people.

The author identifies his six so-called biological types with the six races described by Ludwig Ferdinand Clauss in *Rasse und Seele.* He is not disturbed by the fact

that Clauss's six races include only those of Europe, the Mediterranean, and a part of Asia, whereas his own a priori types seem to exhaust the logical possibilities of classification according to the schema he has adopted.

In attempting to account for the origin of Clauss's races, he seeks to derive each of them from some occupational group and the mental and emotional attitudes suited to it. For example, he thinks the Nordic race (corresponding to the type which he calls "extrapetal") is derived from the primitive hunter; the "fälische" race of Clauss, corresponding to his own "intrahärent" type, is derived from the primitive farmer; the Mediterranean race ("intrafugal" type) from the primitive hand worker; the oriental race ("extrafugal" type) from the nomadic herdsman; the race of Asia minor ("intrapetal" type), including the Jews, from the merchant; and the "ostische" race of Clauss ("intraliquent" type) from the servant, whose sole responsibility is to obey his master.

It does not seem to worry the author that even in primitive times such division of labor as there was must have existed among the different individuals of the same race within the same community. In fact, before the time of rapid transportation and mass production, even such geographic specialization of labor as exists today would have been impossible.

The author is well aware that his a priori method will meet with little approval from those anthropologists who arrive at their descriptions of human races by averaging individual measurements, but he thinks these "empiricists" are self-deceived. They believe their results are entirely objective because they are based on accurate measurements. Actually, they always have in mind a subjective ideal of racial type on the basis of which they select the individuals they measure.

Each of the six races has its own virtues, its own deficiencies, and its own forms of artistic and religious expressions, which the author elaborates in some detail.

The book contains 70 illustrations—many of them reproductions of famous works of art—which the author regards as characteristic of the different races.

West Springfield, Mass. M. F. MARTIN

Caste and Class in a Southern Town. By JOHN DOLLARD. New Haven, Conn., Yale University Press, 1937. Pp. viii, 502.

As stated by the author, "the aim of the study is to grasp and describe the emotional structure which runs parallel to the formal social structure in the community." The material was collected in a five months residence in a small town in the deep South, by means of collecting life histories of negroes, and by interviews with negroes and whites, and personal observation of their behavior. The relations between the class levels of the two castes is first described and the gains of the white middle class as regards economic advantage, sex relations, and prestige. This with the caste patterning in education, politics, and religion, is given as the background for explaining the accommodation attitudes of negroes, aggression within the negro group, the aggression of negroes against whites and of whites against negroes, and the defensive beliefs of the white caste. He then considers the gains of their relationships to the lower-class negroes and ends with a discussion of Class Symbolism and Race Prejudice.

As an exposition of the differences in caste relations between each of the different classes of each other and to those of the other caste, and in describing the local mores, the author makes a real contribution to sociological analysis.

In his interpretation of emotional reactions and their motivation, one is not sure

of the validity of his conclusions. He goes to some length in describing his methodological difficulties and in confessing his recognition of his own bias and how he sought to keep a true perspective. The material is used in an illustrative manner and it is impossible to judge as to its comprehensiveness or how representative it may be. The only answer is the author's statement, "yet I believe that my experience can be repeated, that others can be trained to see what I have seen, and more, and that the construct has predictive value." The whole question of the validity of the methods used, and of whether the interpretations made are warranted by a study of the situation in but one community by a person previously unacquainted with the emotional situation to be studied, raises very fundamental issues which cannot be met in a brief review, but which warrant thorough going discussion by social psychologists if they are to claim scientific validity for this sort of research. The line between high class journalism and adequate testing of hypotheses is not easily determined, and unless there is evidence of the latter any evaluation of the conclusions must rest upon one's knowledge of the ability and integrity of the author.

The author very frankly states his desire to make use of the point of view of psychoanalysis, although he was unable to use its techniques. The psychoanalytic viewpoint runs all through the interpretation, and in several places is used to hazard an interpretation for which no sufficient body of evidence is presented, in a manner which leads one to question whether the "sensitivity" of the author might not well be a little more hard boiled. Thus he says, "There is a form of evidence that white women are interested in the matter of negro sexuality which is quite impressive to me, though I do not know how it will affect others." Would it not be well to ascertain how it does affect other equally competent observers? "My contention is that the white women who told these stories were imaginatively enjoying the freedom attributed to negro women; possibly, though this is more doubtful, they envied the contact with negro men" (p. 169). In other place he says, "In relation to this idea I cannot refrain from guessing that the accusations of the negro men by the white women may oftentimes be a denial of her own excitation directed toward the negro" (p. 333). On such a delicate and highly controversial subject the introduction of "guessing" and of "possibly," "doubtful" hypothesis is inexcusable and inclines one who is not converted to the scientific validity of psychoanalytic 'hunches' to question whether the investigator has not been unduly influenced by them in his own conclusions. On such controversial subjects, the evidence should be imposing before one should risk the discrediting of the scientific study of such important problems, by hazarding objectionable hypotheses.

An appendix of forty pages on "Poor Whites: A Frustrated Class," by Leonard W. Doob, is a valuable introduction to the social psychology of this large class, which it is to be hoped may be a stimulus to numerous studies to test out its hypotheses.

Cornell University DWIGHT SANDERSON

Primitive Intelligence and Environment. By S. D. PORTEUS. New York, Macmillan Co., 1937. Pp. viii, 325.

The basic purpose of this book is a comparison of the mental test performance and of the physical environments of two primitive groups, the aborigines of central Australia and the Bushmen of the Kalahari desert in south Africa. Following a brief

account of the Australian group and its environment (described more fully in *The Psychology of a Primitive People,* 1929), there are nine chapters devoted to a description of the African people, their country and their customs. This material is presented in the form of a travel narrative and is illustrated with many anecdotes. In Chapter XIII is to be found a summary and direct comparison of the two environments. The remaining six chapters in the book contain reports of the psychological investigations. In addition to the main groups of 25 Bushmen and 25 central Australians, other tribes were tested for purposes of control and corroboration, the total number of subjects including 128 aborigines from central and northwestern Australia and 207 south Africans. Although the major comparisons are made on the basis of the Porteus maze tests, many of the groups were also given the Goddard form board, a form recognition and assembling test, a footprint recognition test, tests of rote memory, and an abbreviated form of the Leiter International Performance Scale. Measures of cranial capacity, strength of grip, and handedness were also obtained (Ch. XVIII). A brief summary of Porteus' Hawaiian studies (Ch. XIV), as well as data obtained on several primitive Asiatic groups by a student of Porteus (Ch. XVII), are also included for comparative purposes.

Porteus contends that true racial differences exist, over and above differences which may be accounted for by environmental factors. He is led to this conclusion by the fact that the aborigines of central Australia excelled the Bushmen in mental test performance (the average difference in the Porteus maze test amounting to about 4½ years of "mental age"), although the environment of the former is more "repressive" than that of the latter. This conclusion is open to question on several grounds. Thus, Porteus himself states: "It must, of course, be remembered that the Arunta (central Australia) were in contact with a mission and that some, though by no means all, had attended mission school. The Bushmen, on the other hand, had no contact with missions" (242 f.), and practically no contact with white men of any sort. He goes on to say: "The question then arises as to how the Australians compare with African tribes who have had similar advantages. Fortunately, I am able to supply such comparative data." (243). These comparative data show that the African mission tribes approximate much more closely the performance of the central Australians than that of the Bushmen (257), but Porteus seems to overlook this point. It should also be added that Porteus calls attention to the specificity of mental tests such as the maze, stating, "All we can say of it is that the complex of qualities needed for its performance seem to be valuable in making adjustments to our kind of society" (257). As for the relative "repressiveness" of the two environments, Porteus bases his evaluation on the greater scarcity of food and water supply in central Australia; but he reports that the Bushmen have to "contend not only with some of the most dangerous wild animals on earth, but also with some of the fiercest native tribes," although the Australians are quite unmolested by either. Might not such an environment be equally "repressive" to mental development?

A number of statements appear in various parts of the book which seem to imply logical fallacies. Thus, on p. 13, Porteus objects that "the influence of environment, whether repressive or favorable, has been invoked both ways to explain lack of racial progress." There is no essential inconsistency in such explanations—it frequently happens that both extremes of a factor are unfavorable. For example, it might well be that an *optimum* degree of hardship in the physical environment is the most

"favorable" condition for mental development. On p. 73, Porteus tells us that the term Bushmen "denotes a way of living rather than a race," and later he concludes that the mental test scores of a sample of these people indicate a true racial characteristic. Again, on pp. 213 ff. he seems to argue that since a racial group is merely an aggregation of individuals, then if we are convinced of the fact of individual differences it is reasonable to assume that races also differ. This would only follow if there were a high correlation between the physical characteristics of races and the kind of mental traits in which individual differences have been found.

The material is not presented in very systematic form. In the sections devoted to travel narrative one expects an informal organization, but in the chapters dealing more specifically with the psychological investigations the reader is unduly confused by the somewhat rambling style. Thus, one finds interspersed in more or less random order among the psychological material, passages on geography, ethnology, tribal customs, architectural ruins, travel experiences, and replies to criticisms of Porteus' earlier writings. Probably the chief contribution of the book lies in the interesting and vivid verbal pictures which it presents of the peoples and regions visited by the author.

Barnard College ANNE ANASTASI
Columbia University

Psychology of Personality. By ROSS STAGNER. With a chapter by A. H. Maslow. New York and London, McGraw-Hill Book Co., 1937. Pp. xi, 465.

This book is intended for use as an undergraduate text, by students who have completed a course in general psychology. Dr. Stagner's explicit aims have been to avoid the abnormal and personality "improvement," to develop "a coherent systematic point of view regarding the development of traits," thereby dealing with personality as a "set of concrete, identifiable responses of a behaving organism" and, finally, to concentrate on material applicable to specific individuals rather than to "group minds."

Section I defines the subject matter and describes the "analytic" and "total personality" approaches. Section II, "Descriptive Psychology of Personality," deals with personality structure, the author's views on "acquisition" of personality, attitudes, and so forth. Section III, "Dynamics of Personality," considers appetites and aversions, the theories of Freud, Adler, and Lewin, and the author's "cultural interpretation of motivation." Section IV, "Determinants of Personality," includes a brief treatment of biological factors, and discussions of primary and secondary groups as personality determinants, of the modern American school system and economic conditions, along with Dr. Maslow's chapter, "Personality and Patterns of Culture."

Because of the necessity of limiting the length of the book, much material is presented in almost outline form; for example, sex differences are discussed in a single page. Supplementation is also required by the optimistic assumption that students are familiar with statistical measures (*e.g.* 'r'), biological concepts (chromosomes, location of endocrine organs), and terms of abnormal psychology. Many tables are inadequately annotated for undergraduate comprehension. The number of pages devoted to heredity, biochemistry, and endocrinology totals 18. The treatment of biochemistry is brief and based on outdated material, as is the discussion of endocrinology. No mention, for instance is made of the work of Hoskins or Stockard.

Certain omissions may be noted. No mention is made of the work of Piaget or of Moreno, which, in view of the emphasis on the influence of society on personality, would be appropriate. The Freudian material, ably presented, takes no account of the work with real children (such as Isaacs'), nor of the modifications in terms of social reality by such writers as Horney and Fromm. Lewin's motivational theory is discussed without mention of the concepts of ego- and aspiration-level. There is no section on genius, traditionally a problem in this field. On the other hand, many will object to the extended discussion of technical details of personality test, as disproportionate.

Possibly the main feature of the book is its emphasis on the importance of social conditions in modifying and determining personality. Considerable space is devoted to an interesting consideration of the family, of the relationships in play and recreation in childhood, and of the influence of the school system and economic conditions on the development of personality. With great cogency, he indicates the growing significance of secondary groups, such as the school. In several sections attention is deliberately drawn to "those aspects of society which are harmful to personality," so that they may be marked for change by social therapy. One wishes that this important and significant departure rested more on such material as *Middletown*, and not so largely on semi-anecdotal case material, apparently rather hastily assembled.

Dr. Maslow's chapter is concerned, primarily, with the wide range of "adjustments" that various cultures show to be possible and the bearing of this on possibilities for adjustment in our own culture, particularly by means of shifting from one sub-group to another as a therapeutic measure.

Sarah Lawrence College LAWRENCE JOSEPH STONE

An Introduction to Abnormal Psychology. By V. E. FISHER. 2nd ed. New York, Macmillan Co., 1937. Pp. xii, 533.

The general pattern of the present volume does not differ markedly from that of the author's previous (1929) text. The revision has consisted in the enlarging of the book together with certain notable omissions and changes. The older volume contained eighteen chapters and 512 pages. There are twenty-three chapters in the new. The essential changes are: the addition of a chapter on "Criteria of Personality Adjustment;" an expansion of the material on "Common Modes of Reaction to Difficulties" so that the four chapters (formerly three) devoted to this topic now deal in turn with (1) adjustive reactions, (2) partially adjustive modes of reaction; (3) non-adjustive reactions, and (4) maladjustive reactions; the setting aside of a complete chapter to the anxiety neurosis (as distinct from neurasthenia); and one to the phobias; the addition of a chapter on the "borderline" conditions of psychopathic personality and epilepsy; and a very much needed chapter on certain of the organic psychoses. A final brief chapter on mental hygiene has been added. Dropped from the 1929 book are the first chapter on "Some Typical Problems in Abnormal Psychology" (which is something of a pity as it was one of the more entertaining sections and probably had more "student-appeal" than any section in the revision); the chapter on "The Conscious, Subconscious, Coconscious, and Unconscious;" and the special section devoted to the "sexual neuroses."

Mechanically, the book has unquestionably been improved; the page-size is larger and the print more pleasing. The author's clinical experience presumably having

been enriched in the intervening years, the amount of case material has been greatly extended. Except for very minor alterations certain of the chapters, *e.g.* those on dreams and feeblemindedness, are merely reprinted.

However sweeping the changes noted appear to be, nothing has occurred to remove it from what is essentially an antique tradition in abnormal psychology. The author is mainly concerned with indoctrination and to that end he has constructed a rational system, founded on some assumed characteristics of normal personality. Certain preconceptions concerning the nature of motivation also loom large in the scheme. There is developed an attendant lingo which, while adhered to with considerable consistency throughout the volume, is so tenaciously in evidence as to render even some of the more simple and obvious statements of fact difficult of understanding on first reading. The entire book, that is to say, is an interpretative effort and if one happens to quarrel with the premises of the author he will find much with which to disagree throughout the book. If one's biases prevent him from acceding to the claim that introversion-extraversion, ascendance-submission, and cycloid-schizoid characteristics (the three chief personality variants according to the author) are the best concepts invented by psychologists to describe human nature or that "saturation of consciousness" is a valid descriptive term he will inevitably find the book annoying.

There is, to be sure, a pedagogical position that favors the extreme indoctrination of beginning students and there are practical considerations that make the view inviting. But, at the present stage of development (or undevelopment) of abnormal psychology it is highly doubtful whether rational systems, even as teaching devices, should be the order of the day. As a division of knowledge, abnormal psychology has its descriptive and interpretative problems. It has been the reviewer's experience that a straightforward acquainting of the student with these problems, particularly those susceptible of experimental solution, lends dignity to the field. A factual as opposed to a speculative abnormal psychology cannot be made to spring, Athene-like, full-fledged from the forehead of Jove, but the suspicion is strong that abnormal psychology written in the spirit of current general and experimental psychology will one day dominate the field.

University of Virginia FRANK A. GELDARD

Yoga; A Scientific Evaluation. By KOVOOR T. BEHANAN. New York, Macmillan Co., 1937. Pp. xviii, 270.

While a Sterling Fellow from Yale, Dr. Behanan studied the technique of Yogic practices in India. On his return to Yale to complete the doctorate he was the subject of physiological and psychological experiments to determine what effect, if any, yogic practices have on mental functions. The present study is regarded as exploratory work with no claims to conclusiveness.

Nothwithstanding its modest temper the book is a substantial contribution. Its primary purpose is to reveal the metaphysical and metapsychological theories underlying yoga and the goal of its practitioners. The historical background, the philosophical theories, and the basic ethical system are presented objectively but with sympathy. Mystics and 'yearners' will be disappointed at the practical and essentially modern and critical attitude the writer takes toward the supernatural.

A large part of the book is not directly the concern of the psychologist, which is chiefly limited to the chapters on Yoga psychology, Yoga and psychoanalysis, exercises

is concentration, and Yoga, an appraisal. The temper of Yoga is metaphysical rather than scientific. The chief purpose of all knowledge and practice is a systematic training—physical, mental, and moral—that man may achieve emancipation from the material world. Dr. Behanan calls Yoga "cosmic therapeutics." He thinks that the underlying mystical theories cannot be proved or disproved but he believes that the results of yogic practice upon the body can be measured. He regards "mind as a function of the integrated organism," but the organism turns out to be "an inseparable but distinguishable unit of physical and mental factors." Thus the author slips into the old mind-body dualism.

To measure the results of Yogic practice upon the body, Behanan used the methods of mental testing, himself the only subject. Five tests were used to measure the degree of mental change induced by (1) thirty minutes of yogic breathing and by (2) thirty minutes of breathing followed by twenty minutes of concentration. The tests are named only as adding, code, color naming, coördination, and Chinese puzzle tests. No details of procedure are given and the results presented are regrettably meager. The main conclusions are that "breathing and concentration tend to decrease the mental functions," that yogic practices do not influence intellectual pursuits either favorably or unfavorably, and that yogic breathing produces a state of relaxation which, during concentration, is so complete as to make the subject "forget the presence of the body." The author agrees with James that a profound change in the gearing of his own mental machinery has resulted from yogic practice. Yoga's "success in developing a healthy emotional equilibrium is empirically verifiable." These are personal expressions unsupported by objective data.

Throughout the book the writer's exposition of history and theory is critical and thorough. The evidence of the effect of yoga on the mental functions is limited, but the conclusions drawn are tentative and reserved. In the opinion of the reviewer, the best chapter in the book is 'Yoga and psychoanalysis.'

Ithaca, New York MARGARET RUSSELL BENTLEY

Consistency of Auditory Acuity. By JENNIE LORENZ. Privately Printed, N.Y., 1936. Pp. 162.

The fundamental purpose of the present investigation was the comparison of variability in hearing of 28 school children. One half of the cases had impaired hearing whereas the other half possessed normal acuity, and the two groups were matched for age, sex, I.Q., and racial stock. Two tests were given morning and afternoon, some five hours apart, and repeated in a like manner three days later, on the eight octave frequencies from 64 to 8192 cycles.

It is interesting to note that a rather high degree of consistency was found, with some tendency for the normal to be more so than those with impaired hearing. From the viewpoint of establishing a standard test-situation, these results would indicate that moderate quiet is sufficient for ordinary clinical as well as experimental purposes, whereas factors such as initial practice effects, and perhaps even I.Q., may be of greater significance in producing variability. The highly detailed statistical treatment of the data, however, tends to confuse rather than clarify, and a repetition of the study with a much larger group of subjects is clearly indicated.

Well annotated with a comprehensive bibliography of 213 titles, the author in a review of the field suggests an important program of research which still awaits

experimental test. There is also discussed in detail the history of auditory tests, both of the techniques employed and the development of instrumentation including the audiometers, which should be of considerable interest to workers in this field. The findings of previous investigators unfortunately are presented in a highly uncritical, synoptic fashion; a rehashing which well might have been omitted.

Considering the various methods employed in giving hearing tests, one is not surprised by the inconsistent findings reported by some of the investigators. As yet no policy has been established for the control of factors such as amount of preliminary training, number of series of loud and weak sounds, their order of presentation, number of tests with near-liminal intensities, and the like. While such energy has been expended in the past few years upon the study of the neural phenomena of hearing, little if any consideration has been given to these perhaps more prosaic, but certainly no less important, problems. Since the development of inexpensive instruments, particularly the new audiometers, which require no batteries and are capable of tests at practically all audible frequencies, there is every reason, clinically as well as experimentally, for the immediate establishment of a standard hearing test. A round table symposium at the next annual A.P.A. meeting devoted to this question would be well worth the time and effort. One of the difficulties in evaluating the literature at the present time lies in the inability to make satisfactory comparisons because of differences in test-procedures employed. A similar state of affairs in the field of intelligence testing would not be permitted to continue in this fashion uncorrected. These problems which make themselves felt even upon a cursory reading of the present monograph far outweigh the consideration of the experiment-proper, which might well have been condensed in size for publication in one of the standard experimental journals.

Brooklyn College EDWARD GIRDEN

The Mind of Man. By WALTER BROMBERG. New York and London, Harper & Brothers, 1937. Pp. 323.

Sheer enjoyment of this well-written history of psychotherapy might have inhibited the reviewers critical attitude, had not the author's soundly scientific approach to his popularly presented subject been obvious. The value of the book for psychologists is that it integrates in accessible form the whole field of psychiatry, with modern psychological implications. It will be admirable collateral reading for students of abnormal psychology and psychology of religion.

In spite of the similarity of neurotic symptoms and causes, and a certain essential sameness in the psychology of the healing methods through the ages, it is apparent that forms of aberration have changed with changing social conditions. From primitive magic, through superstitious beliefs and practises connected with Christian theology, when the insane were kept chained in filthy surroundings and devils were exorcised from "witches" and other neurotics by torture and priest-craft, belief that cure of the insane was possible through humane and hygienic methods did not emerge till the nineteenth century. Charcot brought to clinical dignity the hypnosis of Mesmer, and Bernheim applied suggestion as a curative agency through hypnosis. Freud found that disassociated memories recalled under hypnosis seemed to have a causal relation to hysteria and that bringing them to consciousness—"talking them out," relieved the symptoms. He found that these disassociations usually concerned un-

pleasant or repressed sex experiences. Adler found that the basic conflict was between the individual's desire for power and the restraints of environment or physique, while Jung found it deeply rooted in biological and religious urges. All these approaches used the technique of psychoanalysis as a corrective agency. The chapter on this treatment is specially specific in presentation of both principles and practice.

As for the future, psychiatry has trained itself to look at a human being as a unified organism, with body and mind interrelated, and in close relationship with his social and physical environment. The use of insulin for dementia praecox and of malarial blood for general paresis points the way for further medical research. The most forward-looking tendency is that of attention to the relation of society to the individual. The shaping of social ideals by the utilization of the knowledge of inner psychologic life acquired through a century of psychiatric practice is a recognizable goal.

The book has a very extensive and well-classified bibliography, illustrations, and pertinent case records from the author's files amplify the theories presented.

Ann Arbor, Michigan BERENICE BARNES SHEPARD

Later Criminal Careers. By SHELDON and ELEANOR GLUECK. New York, The Commonwealth Fund; Long, Humphrey Milford, Oxford University Press, 1937. Pp. xi, 403.

This volume reports the first of successive five-year periods of follow-up investigations of *500 Criminal Careers* of the Massachusetts Reformatory. The authors hope to continue the study through the life cycle of this particular group of criminals. The writers give a point by point comparison of the group after a lapse of five years.

The results are complicated and the conclusions and comparisons marked by many exceptions both in trends and in individual behavior; to give a summary of the findings in a few words would be misleading and will not, therefore, be attempted. The study is a model of patient descriptive detail and long-time follow-up still to be continued. It is precisely the type of descriptive content we need instead of the fuzzy psycho-penosocio-psychiatric explanatory accounts that have flooded our magazines. For some time to come penology can well afford to let description outstrip explanation.

On the whole, the authors find that the biological, sociological, and psychological patterns remain unchanged in this second five-year period, but there is some change for the better which is attributed to the factor of aging, recalling Dr. Cabot's statement several years ago, "Perhaps this type of crime [larceny, robbery and breaking and entry] is a symptom of a self-limited disease of personality which ordinarily runs its course during the years from sixteen to thirty-five or forty." The Gluecks seem to have found substantial facts for Cabot's impressionistic observation, although after the age of 36 years "there is retrogression to less favorable conditions." The authors suggest that the proper use of psychiatry might hasten the ripening of insight. They are cautious in their conclusions, however, stating that "whether the component elements of this process [aging] are largely biological or psychological or social is a question which we cannot answer within the confines of the present research."

As in *500 Criminal Careers,* a chapter is devoted to predicting criminal conduct in terms of cumulative statistical data.

The book is outstanding and should be read by everyone interested in penology:

the intellectual layman, ministers, social workers, physicians, and psychologists. As the factual data is clearly stated, one is given ample opportunity to draw his own conclusions and reflect. May subsequent follow-ups be duly forthcoming.

University of Pittsburgh — W. T. ROOT

Educational, Psychological and Personality Tests of 1936, Including a Bibliography and Book Review Digest of Measurement Books and Monographs of 1933-36. By OSCAR K. BUROS. New Brunswick, N.J., School of Education, Rutgers University, 1937. Pp. 141.

Of such a work as this, we must ask, is it complete? Is it accurate? On both counts the work is acceptable. The bibliography lists 365 tests, almost all of the year 1936. For each test, the following information is given: title, description of the group for which the test was constructed, date of copyright or publication, individual or group test, number of forms, cost, time, author, publisher, and references. This last term reports the references, when known, on the construction, validation, and use of each test. Since ninety out of every hundred tests published in the United States should be withdrawn because they have never been satisfactorily validated and standardized, Professor Buros's annual publication would be made much more useful, if he would mark with a prominent star those which were valid, reliable, and had satisfactory norms. Then busy workers could neglect the rest, or if they wasted their money on "gold bricks," the fault would be their own.

The last two-thirds of the monograph are devoted to a "Bibliography and Book Review Digest of Measurement Books and Monographs of 1933-36." Presumably, now that he has both sections up-to-date, the next number will deal only with the tests and books of 1937. This review section is very interesting. Instead of reviewing the books himself, he has quoted from reviews of them in the technical journals. They show that reviewing of such works is at a low ebb on the North American continent. Few reviewers are as courageous as Conrad who says of John O'Connor's *Psychometrics* "The book represents a degradation of editorial responsibility by its publishers." Yet other reviewers failed to mention its obvious defects. If we could wait until Buros assembles his reviews of the 1937 publications, our libraries would not be cluttered up to the extent they undoubtedly will be with the useless books of that year. Reviewers should read this second section of the monograph and take their responsibilities more seriously.

Since the publishers of this monograph are anxious to learn if it is of value or not, may I say that, in my opinion, it is an extremely useful publication whose value would be increased by the adoption of the suggestions given above. The series should undoubtedly be continued.

University of Toronto — PETER SANDIFORD

La naissance de l'intelligence chez l'enfant (Vol. I). By JEAN PIAGET. Neuchatel and Paris, Delachaux & Niestlé, 1935. Pp. 429.

This volume, which is the first of a series of three, presents the results and interpretations of a study of infant behavior during the first two years of life. It is an extension of the earlier studies on *Le langage et la pensée chez l'enfant* and *Le jugement et le raisonnement chez l'enfant.*

The author follows those theorists who regard 'intelligence' as adaptation. He defines adaptation as "un équilibre entre l'assimilation et l'accommodation." Thus "dans un acte vrai d'intelligence, le besoin qui serte de moteur ne consiste pas seulement à répéter, mais à adapter, c'est-à-dire à assimiler une situation nouvelle à des schèmes anciens et à accommoder ces schèmes aux circonstances nouvelles" (p. 187). Six levels of development, representing a continuous though distinctive progression, are outlined as (1) exercise of the reflexes; (2) first acquired adaptations and primary circular reaction; (3) secondary circular reactions and behavior designed to prolong interesting scenes and events; (4) coördination of secondary schemes and their application to new situations; (5) tertiary circular reaction and the discovery of new means by active experimentation; and (6) invention of new means by mental combination. The first two of these levels are classified as elementary sensory-motor adaptations, the remaining four as intentional adaptations.

Although amply illustrated by nearly two hundred observations of infant behavior, this carefully elaborated account does not differ substantially from those of earlier writers who concerned themselves with teleological speculations on the development of intellectual life. The informed reader will easily recognize the heavy influence of J. M. Baldwin upon Piaget. A sound descriptive psychology of the development of psychological performance still remains to be written.

Cornell University CLEO CHRISOF

Hypnotic Power. By COLIN BENNETT. New York, E. P. Dutton & Co., 1937. Pp. xi, 158.

The author of this volume is obviously British, obviously not a physician or a psychologist, and equally obviously not a charlatan. He has written a brief manual of practice which is characteristically the product of the interested and reasonably well-informed layman. In scope, the treatment ranges from hypnotism as a casual parlor amusement, through stage hypnotism, and on to its therapeutic uses. Its best features rest upon a wide practical acquaintance with hypnotic phenomena, reënforced by certain amounts of careful reading. Its worst features are directly traceable to a layman's impatience with precautions, coupled with an eager belief that anyone innocent of medical or psychological training may use it without hesitation in 'curing' sundry neurotic symptoms.

The reviewer is tempted to quote at length, but one or two brief samples may suffice to reveal the general level of information represented. Thus we learn that pressing the point of the jaw and back of the head simultaneously "has the effect of compressing the semicircular canal and momentarily impeding consciousness." We are told that "many diseases of the functional and nervous kind can be harmlessly relieved and cured by dealing with their symptoms. Typical instances would be: stammering, hiccuping, sleeplessness, nervous loss of voice, neuralgia, constipation." The readers of this JOURNAL will need no reference to the author's implicit belief in the value of 'passes,' to his frequent references to magnetism, to his admixture of psychoanalysis and hypnosis for administration by laymen, to be convinced that wide circulation of the volume among the untrained folk for whom it was written will produce effects other than the beneficent outcome sincerely desired by the author.

Cornell University JOHN G. JENKINS

Bewusstseinstypen und ihre Beziehung zur Psychopathologie. By J. H. VAN DER HOOP. Huber, Bern, 1937. Pp. 375.

In his book, which has sprung from many years of labor in the field of psychotherapy, the author wants to describe the types of consciousness—or rather of personality. According to him, Adler reacted too strongly against Freud in rejecting unconscious motivation. Jung compromised and accepted both conscious and unconscious motivation, but relied too strongly on his distinction between the introverted and the extraverted personality. The author's doctrine is that everyone is both, introverted and extraverted. Naturally this causes complication in the description. But Kretschmer's method is even more complex and has introduced in Germany the term of a many-dimensional diagnostic. The author contrasts his typological explanations with the psychoanalytical ones. The relation between the two is treated in detail in the second, the clinical part of the book. The whole book is divided into three parts, the first treating the problems as related to psychology, the second to psychiatry and the third to philosophy. The author inclines toward four particular classes of personality types, which he calls the instinctive, the intuitive, the thinking and the feeling types. Applying to this four-fold division the other of extraversion and introversion, he has an eight-fold scheme under which he tries to describe in a very abstract manner, with scarcely any concrete biographical examples, the psychology, psychiatry and philosophy of types in his very large, very well printed, nearly four hundred pages.

Coral Gables, Florida — MAX F. MEYER

Child Psychology. By NOEL B. CUFF. Louisville, Kentucky, Standard Printing Co., 1937. Pp. 299.

Apart from the attractive format, little can be said in praise of this addition to the steadily growing list of elementary text-books on child psychology. The author shows slight familiarity with or understanding of the experimental literature in the field; one gains the impression that his reading has been largely confined to secondary sources which have not always been well assimilated. Careless statements, unjustified conclusions, and inaccurate quotations abound throughout the book. Some of these are indeed curious as in Table IV, on p. 107 in which "Restaurant" is cited as one of the stimuli used by Sherman in his study of the emotional responses of new-born infants.

The fifteen chapter heads comprise, so the author states, the fifteen topics most often selected by teachers and students of child psychology for inclusion in an elementary text book. That these topics do not readily lend themselves to a systematic treatment of the field should surprise no one who understands the difference between mass statistics and individual instances. Each chapter is followed by a list of topics for discussion, suggestions for individual "experiments" a set of objective-examination questions and a list of references.

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